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I. A PAGE OF SCIENTIFIC HISTORY:

REMINISCENCES OF THE EARLY DAYS OF THE ROYAL COLLEGE OF CHEMISTRY.

By A. W. HOFMANN, Ph.D., LL.D., F.R.S., &c.

THE Royal College of Chemistry owes its origin to the grand development of chemical science which took place towards the middle of this century, more especially due to the labours of Professor Liebig, then teaching chemistry in the small University of Giessen. At that time Liebig had already written his "Chemistry Applied to Agriculture," in which he, for the first time, had laid the true scientific foundation of this most important of all human trades. His work on "Animal Chemistry," which gave a new start to physiological and pathological researches, had just appeared, and his "Familiar Letters on Chemistry" had opened to the many a field of scientific instruction which hitherto had been accessible but to few. But Liebig had done more than this. He had founded a school of chemistry—indeed, the first school of chemistry. Between the years 1835 and 1845, the University of Giessen possessed the only laboratory in which not only chemical analysis, but also the art of performing chemical researches, more especially in the department of organic chemistry, was systematically taught. Young men from all quarters of the globe flocked to Giessen for the purpose of studying under Liebig's auspices, and the number of important chemical investigations which at that period emanated from the Giessen laboratory might with truth be said to form an era in the history of scientific discovery.

It was more especially in England that the progress of Liebig's school had been watched with deep interest. English agriculture and English industry had obviously received a powerful impulse from the researches carried out by Liebig and his pupils, and the idea very naturally suggested itself that the establishment of a similar school in England would materially accelerate their further progress.

The utility of founding a chemical school upon the model of Liebig's, in Great Britain being once admitted, it was not long before a number of public spirited men formed an association for the purpose of calling such a school into existence. To Dr. John Gardner, the translator of Liebig's "Familiar Letters," belongs the merit of having agitated the project with great ardour and indefatigable perseverance. Temporary premises were taken in St. Martin's Place, where a public meeting was held on the 29th of July, 1845, at which a definite form was given to the new institution by the election of a council, with power to add to their number, and by the appointment of certain executive officers under the control of the council.

Sir James Clark had joined the movement from its very beginning. He soon became deeply interested in the new institution, the staunchest and most faithful friend of which he remained as long as he lived.

The first step the council had to take was to appoint a fit professor capable of organising the school to be established, and, from this early period, the influence of Sir James began to be exerted. He at once most clearly recognised what was actually wanting. There was no lack of most excellent chemical lecturers in England; indeed, the style of experimental illustrations, then quite general in England, was infinitely superior to that which at that period prevailed in Germany and on the Continent in general. It was easily accessible instruction in chemical analysis, and more especially in the art of carrying out experimental inquiries, that had to be provided; the few laboratories at which one or two young men were received as practical students being altogether inadequate to supply the daily increasing demand for instruction on moderate terms, such as was offered by the chemical laboratory of the University of Giessen. After mature consideration, the council of the new college, chiefly at the suggestion of Sir James, agreed that Liebig should be called upon to recommend one of his assistants for the new professorship. The council did not conceal to themselves the difficulties involved in the appointment of a foreigner, but none of the distinguished young English chemists of that period, though nearly all pupils of Liebig, had had the advantage of being thoroughly acquainted with the method of teaching introduced by Liebig—in one word, of having taught under the great master.

Prof. Liebig recommended to the council three of his former assistants, viz., in the first place, Dr. Will, then Assistant Professor in the University of Giessen; in the second place, Dr. Fresenius, Professor of Chemistry in the Agricultural School of Wiesbaden; and, lastly, the present writer, then one of the junior teachers in the University of Bonn. Drs. Will and Fresenius declined the professorship offered to them in the first instance, but the writer of these lines could not resist the temptation thus thrown in his way.

At an interview with Dr. Gardner, who had become secretary to the college, and who came over to Bonn for this purpose, the preliminaries of an agreement were settled. But some very important difficulties had still to be removed. The council being unable to offer more than a definite appointment for two years, the writer, who had never been in England, was unwilling entirely to break his connection with the University of Bonn. It was but natural that he should have wished to retain a hold of his most modest position in Germany in case his experimental mission to England were to fail. No one, except a man in Sir James's influential position, could have overcome this obstacle.

In the latter part of the summer of 1845 the Queen and Prince Albert visited Germany. They were in Bonn on the day the monument of Beethoven was inaugurated. It was on the very same day that I met Sir James Clark for the first time. Our interview did not last more than ten minutes, but they were sufficient to shape the course of my life. The assurances Sir James gave me removed the last hesitation which I felt. I was introduced to the Prince, who confirmed every word Sir James had said. To bring matters to a conclusion, Sir James asked me to come next day to the castle of Brühl, where the English Court was residing with the King of Prussia. At this interview the Chevalier Bunsen was present.

This interview is a bright spot in my recollections, and I may perhaps be excused for entering into some details. Twenty-five years have elapsed, but I could almost repeat the words which were spoken. "After a short stay in London," Sir James said to me, "you will probably be amused at your having been clinging with so much tenacity to your private docentship at Bonn; still, we find this feeling perfectly legitimate, and we think that only with your mind perfectly at rest as to the future, can you devote your whole energy to the task, by no means easy, which you are going to undertake. You ask that we may procure for you from the Prussian Government a leave of absence for two years, that you may be enabled, after the lapse of this term, to resume the position you now hold in case you should not succeed in establishing a school of chemistry in

London. This we are most willing to do, but we believe that we ought to do more for you—we ought to obtain a promise from the Prussian Government that, should you feel disposed to return to Germany after two years, you should be allowed to join the University of Bonn with such promotion as you would in all probability have attained by remaining in Bonn. What would your position probably be after two years?" I said that if I had good luck, I should then hold an Extraordinary Professorship. "This is exactly what the Chevalier Bunsen says," replied Sir James, "and we must take care to make them promise that they will appoint you Professor Extraordinary when you return." The Chevalier Bunsen said that it was a very uncommon thing to give promises of this kind, but that if Prince Albert were to speak upon the subject to the King, the demand might after all be granted. He himself meanwhile undertook to submit the case to the minister." I said that if they succeeded in carrying out this scheme I should be ready to go to-morrow. This was really a day of good luck. The great state transaction which was to accomplish so important a result was carried on with unheard-of despatch. The King, the Prince, and the minister were, in fact, close at hand; they all, together with my two new protectors, being inmates of the castle of Brühl. After two hours, during which I took a charming morning walk in the beautiful gardens of the castle, we met again. When I entered Sir James's room, I found him beaming with delight. His mission had been perfectly successful. The Prince and the King had discussed the question, and the latter agreed to everything the former had demanded. The Chevalier Bunsen in his interview with the minister had found more difficulties. "The minister," he said, "cannot promise to make an appointment after the lapse of two years, because he does not know whether at that time he may be still in office. All he can do for Dr. Hofmann will be this. He can at once appoint him to an Extraordinary Professorship in the University of Bonn, and then give him leave of absence for two years." I had no objection to this mode of getting over the difficulty.

In October, 1845, the analytical course of the College of Chemistry commenced, a temporary laboratory having been established in Great George Street, Hanover Square. A considerable number of students (about twenty) at once entered, and afforded sufficient evidence that an institution like the one projected was really wanting in England.

The opening of the school in these temporary laboratories having been effected, the council proceeded to look forward to a more convenient and permanent habitation for the college.

Encouraged by the promises of support they had received, and backed by the guarantee of several noblemen and gentlemen, the council took premises in Hanover Square. This site not only possessed the advantages of a good and ostensible position, affording all necessary accommodation for the official purposes of the college, the residence of the professor and secretary, and the meetings of the members, but supplied the desideratum of a large piece of ground, with a frontage in Oxford Street, whereon convenient and well arranged laboratories could be erected.

To the construction of these laboratories the most anxious consideration of the council was next directed. After lengthened consultations with their architect, Mr. James Lockyer, a plan was ultimately adopted to which all the parties concerned had given their adhesion, and on the 16th of June, 1846, the council had the gratification of seeing the first stone of the building laid by H.R.H. Prince Albert, who had never ceased to lend his most cordial support to the new establishment. In the short period of three months the new building was completed, so that as early as October, the operations of the school, to which in the meantime the patronage of the Prince had procured the title of the Royal College of Chemistry, could be commenced in the new premises.

As might have been expected, in the endeavour to institute such an establishment as the Royal College of Chemistry, serious outlay had been unavoidable. Here the difficulty of the task commenced. In building the laboratories and furnishing them with the necessary apparatus, the responsibility of a heavy debt was incurred. A building fund had been opened for this purpose, and contributions at first flowed in with great liberality, but long before the buildings were completed the current slackened, and at last a debt of £2000 remained, without apparent means of defraying it. The pressing demand was met by the noble generosity of the council, who, by a *pro rata* contribution among themselves, succeeded in extinguishing this heavy debt. But this effort was by no means sufficient to clear the difficulties. It was found that the ordinary expenditure of the institution exceeded, and, indeed, very considerably exceeded, its income. The annual subscriptions declined, and notwithstanding the constantly increasing receipts from the fees of the students, the council of the college became more and more embarrassed for the means necessary to carry out the noble work so well begun. The most conflicting proposals were made to restore the financial equilibrium of the institution.

It was at this dangerous period, when the very existence of the college seemed imperilled, that Sir James stood forth its most enlightened and its most devoted champion.

It had been the ambition of the council to establish in England a school of chemistry on the model of the chemical laboratories connected with the German universities. They were well aware that not one of those institutions is self-supporting. All these institutions receive very considerable support (varying, of course, with the importance of the university) from the respective states to which they belong.*

The British Government of that period did not yet recognise the claims of science, and it was therefore necessary to look towards other sources from which to defray the expenses of the college. For this purpose the system of life subscriptions and annual subscriptions had been resorted to. But since it was not likely that many would be found willing to support on public grounds alone an institution designed for the diffusion of chemical knowledge and for the advancement of science in the country, it became necessary to hold out some prospect of personal and private advantage to the subscribers. Various proposals were made for this purpose. One of the privileges which from the very commencement it had been thought possible to grant, was the analysis of substances sent by subscribers. Some of the members of the council were most anxious to establish in the college an analytical department undertaking private investigations on a very large scale. They hoped to secure in this manner ample means for carrying on the scientific school. Others, again, imagined that an institution midway between the Royal Institution and the Polytechnic would attract crowds of members, and that the income derived from brilliant exhibitions, evening lectures both instructive and amusing, and, lastly, from frequent conversaziones, would be more than sufficient to defray the expenses of the school. All this stupendous machinery was to be put in motion by the Professor of the College with his two assistants, who were, moreover, expected to instruct some thirty or forty students in analysis, and to contribute to the progress of science by their researches. Happily, there were men on the council who clearly perceived the folly of these proposals, which, if they had been entertained, would have altogether changed the nature of the institution, and thus have deprived the country of the only school of practical chemistry then in existence. If these several proposals, which were urgently pressed by some well-meaning but ill-advised members, were rejected, it was due to the clear-sightedness of Sir James, who, aided by some other influential members, more especially Lord Ashburton, Mr. John Dalrymple, and, at a later period, by Mr. Warren de la Rue and Dr. Bence Jones, succeeded in convincing the council that the only way of saving the college would be by confining themselves to the principal object contemplated in its foundation, viz., the advancement of science by means of practical instruction in the laboratories and by researches. From this moment Sir James became the very soul of the school. By his advice all unnecessary expenditure was at once avoided; the large premises in Hanover Square were given up, the laboratories only being retained; lastly, the office of secretary was established. It was the confidence in Sir James's friendship that induced the Professor to give up half his emoluments.

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* The annual grant of the Prussian Government to the chemical laboratories of the University of Berlin amounts to 7100 thalers (£1065).

By this re-constitution of the College the expenditure was at once very considerably diminished, and since the income from students' fees was still increasing, the council found no difficulty in defraying the small deficiency by an annual donation which the College owed to the liberality of Lord Ashburton, and by the subscriptions of the friends who had remained faithful to the institution. A series of prosperous years now ensued: the College gained more and more the estimation of the public and of the leading scientific men, and Sir James had the satisfaction of seeing that the time and labour which he had so largely devoted to the institution did not remain without reward. But Sir James was not one of those who are satisfied with a partial success. He perceived that the financial balance, which had been happily restored in the College, was very precarious, and easily disturbed by fluctuations in the attendances of pupils or by falling off of subscriptions. The more the College began to fulfil his expectations as a teaching institution, the more was he anxious to establish it on a more permanent foundation. His aim was to obtain a Government grant for the school, and no pains were spared on his part, no means neglected which his high and influential position could suggest to attain this important object. Several attempts failed, and it was not until 1852 that a happy combination of circumstances enabled Sir James to achieve a plan which he had so long cherished. It was in that year that Dr. Lyon Playfair resigned the Professorship he then held in the School of Mines connected with the Museum of Practical Geology in Jermyn Street. Sir James at once placed himself in communication with Sir Henry de la Beche, then Director of the Museum, and by his exertions, in which he was most generously supported by Lord Ashburton, he succeeded in bringing about a fusion of the two institutions. The Royal College of Chemistry was adopted by Government as the chemical department of the Museum of Practical Geology, the Professor of the College becoming the successor of Dr. Playfair as Director of the Laboratory and Chemist to the Museum. The Royal College of Chemistry became the property of Government, under whose auspices it has since been carried on in a most prosperous manner.

Of the further development of the Royal College of Chemistry in connection with the Royal School of Mines, and of the services which it has rendered to the cause of science, nothing need be said here, except that Sir James never ceased to take the most lively interest in its progress. Even long after my return to Germany—indeed, within the last twelve months—I received two letters from him, which showed that he was still watching the welfare of this College with unabated solicitude.

I must not, however, conclude this sketch without alluding in terms of the deepest gratitude to the, I might almost say, paternal kindness with which Sir James overwhelmed me from the moment when first we met at Bonn until when, but shortly before his death, I saw him last. During the many years that I have directed the laboratories of the Royal College of Chemistry, often, and more especially in the beginning, under difficulties which appeared insurmountable, Sir James's ever-ready counsel, his most active support, his warm-hearted sympathy, have never failed me on any occasion. Not without profound emotion can I think of the time which most unsparingly he devoted to my instruction; indeed, so far went his anxiety to promote my progress, that, tired as he often was from the exertions of his professional life, which at that time had reached its culminating point, he did not shrink from giving up the late hours of the evening to correct my early attempts at English composition. The evenings which, twice a week and often more frequently, I spent at that time with Sir James and Lady Clark in Brook Street, belong to the most charming recollections of my life. Indeed, if I have not altogether failed to fulfil the expectations which the originators of the College of Chemistry entertained when they placed the new institution under my direction, it is due to the powerful influence which the friendship of Sir James exercised upon my career at that most important period of my life.

POSTSCRIPT. Having in the preceding sketch pointed out the early exertions which Sir James Clark made to procure a Government grant for the College of Chemistry, it is certainly not without interest to contemplate that at present a Royal Commission is sitting in London charged to inquire in what manner science can be best promoted in England by the grant of Government subvention, and that the author of this sketch, whilst writing it, has received from that Commission an invitation to proceed to England for the purpose of giving evidence on the mode in which scientific institutions are supported by Government in Germany.

II. THE THEORY OF ATMOSPHERIC GERMS.

By ARTHUR ERNEST SANSOM, M.D.,
Member of the Royal College of Physicians.

ONE of the most celebrated of the Deputies lately commenced a speech before the Austrian Reichsrath with the words:—"The great question before us is,—Is Charles Darwin right or wrong?" Probably this sounded strangely, for the world is not accustomed to be told suddenly that the solution of a scientific problem is material to its position or its progress. There is another great question which has been recently debated with a considerable amount of warmth, and has seemed to evoke a very considerable interest. Mighty issues are involved in it, and yet its problems are interwoven with the most ordinary processes of domestic life. It has to do with the most abstruse speculations as to the origin of living things in the kingdom of nature. It concerns the art of the brewer and the maker of wine. It is linked with the processes which preserve food from decomposition and sewage matters from being hurtful to mankind; with the treatment of wounds and with the arrest and prevention of pestilence. Complex in its relations, the question can nevertheless be curtly stated—Do living things of necessity spring from pre-existing living things or no?

Those who debate this question range themselves in two classes, with defined lines of demarcation between them. The ones say: living things do *not* necessarily arise from pre-existing bodies, which possess or did possess attributes of vitality; but they may originate from non-living matter in connection with the ordinary forces of nature. The others say that living things invariably are the progeny of living things; that in nature only vital material is capable of endowing non-vital matter with the special properties which itself possesses. After a protracted debate in the learned societies of France, the upholders of each side of the question appear to have arrived at that stage at which each prefers to remain unconvinced. More recently in this country the ardour of debate has been displayed, and yet there are strong forces in either camp.

The following is a brief outline of the debateable ground. It is well known that it is the tendency of moist organic matter, under certain physical influences, to undergo certain processes known as decay or decomposition. Either it disintegrates, evolving nauseous odours, and giving rise in the course of the disintegration to a multiplicity of chemical products organic or inorganic. Such is known by the term putrefaction. Or, the organic material being less complex in the beginning, the process runs a more definite course and the products are less varied. Such is fermentation.

In both instances there is observed intimately connected with the process, the occurrence, growth, and multiplication of living organisms, either amid the particles of the moist decomposing substance or upon the surface, where these frequently make their appearance as ordinary mildew. How are these results explained by the two theories? According to the one, the molecules of a putrefying or a fermenting body are in a state of motion, tending to the disruption of their elements. The living particles observed are the results of the communion of certain non-living elements with the physical forces with which they are in relation. Thus there is a strict analogy between crystallisation and creation. As in the one case certain molecules under certain conditions assume definite crystalline forms, so certain molecules under other certain conditions assume the appearances and the attributes of vitality.

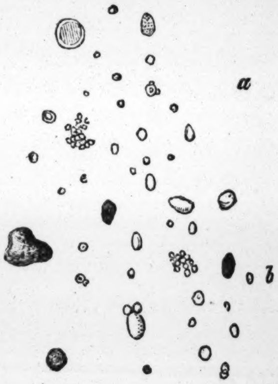
According to the other theory, there is a single cause for all the phenomena. This cause is the presence of living matter. The organic elements of a putrescible or fermentescible compound undergo disruption by no inherent tendency of their particles to motion, but by the influence upon them of living, growing, and multiplying organisms, which, by their very acts of life and struggle for existence, superinduce this disruption. The living beings which are acknowledged to be present are the intimate causes and not the adventitious signs, nor yet merely intermediate agents of the decomposition of the material.

If we investigate the question from the stand-point of the second theory, we shall have to inquire concerning the living organisms declared to be the *prima moventia*, how they are brought into relation with the decomposable matters. The close relation between the presence of atmospheric air and the occurrence of the phenomena of putrefaction has been constantly admitted. It is a matter of common knowledge and common practice that to expel the air from a putrescible substance is a powerful means of preserving it from putrefaction. The presence of air is one of the conditions insisted upon by the supporters of the theory of spontaneous generation as essential to the production of living forms. No other gas can be substituted for atmospheric air except oxygen.

Upon what element of the air does its influence upon putrefaction and upon the appearance of living forms depend? The nitrogen may be at once dismissed, as direct experiment shows that it prevents putrefaction and is fatal to living things. The influence might appear with greater show of probability to be due to the oxygen. This was the hypothesis of Gay-Lussac, but experience soon showed that in many instances putrefaction was prevented when oxygen had free access to putrescible solutions; and when the progress of chemistry allowed Gay-Lussac's experiments to be conducted with greater precision, it was found that in cases wherein putrefaction was arrested by the attempted expulsion of air, oxygen, instead of being invariably absent from the gaseous residuum, was very generally present. Furthermore, according to Dr. Bastian's late experiments, the development of living forms may take place though all air may have been excluded as rigidly as possible.

Schwann concluded from his experiments that it is not oxygen, at least the oxygen of the air, which occasions putrefaction, but *a principle* contained in ordinary air which heat can destroy. This same principle could also, according to Schroeder and Dusch, be arrested by the meshes of cotton-wool. And it could be arrested by flexures made in a fine glass tube which admitted the air to a putrescible fluid, according to observations made by Pasteur and more lately repeated by Lister. Now, it may be urged that these results are not the *invariable* teachings of experiments. In case of certain putrescible fluids, and in certain conditions, putrescence has not been prevented by the means employed; but it must be recollected that, in all the instances quoted, parallel experiments were carefully made. In the one case, wherein the air was uncalcined or unfiltered, putrescence took place and organisms appeared; in the other, wherein the air was calcined or filtered, there were no putrescence and no organisation. The first step in the inquiry is the determination of the substances besides the gaseous constituents which are present in atmospheric air. The suspended matters are proved to be—(1) A number of fine particles of inorganic matter; chloride of sodium derived from the sea is proved by spectrum analysis to be present almost constantly; fine particles of the metals are present, especially in localities where are prosecuted trades, wherein they are manipulated. (2) A quantity of organic *débris*; starch cells and fragments of vegetable tissue are common; cotton and wool fibres are found in certain localities besides many kindred dry organic substances. (3) Organic *débris* derived from the animal body is also abundantly found. Epithelium cells have been frequently observed in ill-ventilated rooms; Eiselt found pus cells in the air of an ophthalmic hospital; organic matter, moreover, is given off from the lungs, for sulphuric acid is darkened, permanganate solution is decolourised and pure water rendered offensive by it. (4) Lastly, organised bodies, spores of fungi, have been seen by a host of observers. The appearance of organised bodies in ordinary atmospheric air was first satisfactorily established by Pasteur (see fig. 1). Since his observation the microscopic examination of air has been made in many ways by different observers, and all have agreed as to the presence in it of sporules of cryptogams, and of bodies possessed of vitality.

FIG. 1.



Corpuscles obtained from atmospheric air, treated by (*a*) solution of potash, (*b*) aqueous solution of iodine. (After Pasteur.)

The fact of the presence of organised bodies in the air being to all intents and purposes uncontested, we have to consider their possible relations in the causation of the development of vital forms in putrescible solutions. On this point there is wide difference of opinion. M. Pasteur, with the Panspermatists, holds that they are the sources of all the organisation; M. Pouchet and the heterogenists, that they are far too infrequent and insufficient to account for the phenomena.

The question, "Are the organisms which have been observed and described sufficient to account for the multitudes of living forms which are met with in the course of putrefaction?" is of the first importance. If so, they ought to be observed upon or within the putrescible solution, and their mode of multiplication should be distinctly made out. It is, however, certain that this cannot be established. When an organic solution is examined, prior to the commencement of putrefaction, the highest powers of the microscope fail to detect the ova or spores of the future minute organisms. There remains, therefore, this dilemma: either the germs are invisible or else the primigenetic molecules are self-formed in the fluid. We should then inquire if there is anything in analogical evidence which could tend to the conclusion that the germs of living things are so minute that the microscope cannot detect them. Dr. Beale, using the highest powers, figures extremely minute masses of germinal matter which develop into perfect fungi, yet which might elude very close investigation; and it is far from certain that these are not the indices of still more minute particles of living matter. It is surely unfair to stay further investigation by asserting at once that, because the germs cannot be demonstrated, therefore the inquiry is at an end, and the question is judged. The heterogenists have constantly urged this: they say "Show me a specimen of these creatures and I will admit them; otherwise I shall declare that they do not exist." The eye alone is not the test absolute of the cause of phenomena. Would a philosopher assert that a man could safely enter a poisonous atmosphere because he could see nothing wrong even with the highest aid to his visual powers? Would it be fair to conclude after the inoculation of vaccine virus into an animal that no virus exists in its blood because none can be detected? Would it be fair to infer that the weeds which appear in such abundance in our gardens spring up spontaneously because we cannot trace their seeds as they are wafted by the air? The method which Professor Tyndall has popularised has shown that the apparently homogeneous atmosphere which we breathe is dense with floating particles; the minuteness of these particles is extreme. It has been asserted that though many filters fail to intercept them, a stratum of cotton-wool is a perfectly efficient filter; but we must not entirely accept this dictum, for Pasteur long ago found that if strata of cotton-wool were arranged in series and air were drawn through them, fine particles were absorbed by many of the successive layers. As far, then, as the direct examination of the air, microscopic or otherwise, is concerned, we may conclude that there is at least as much evidence in favour of the germ theory as of the spontaneous generation theory.

It is at least as easy to conceive the existence of germs so minute that vision, however aided, fails to detect them, as it is to conceive the undemonstrable union of ordinary matter and ordinary forces to produce a living thing which, when produced, overrides the laws of both. Two objections, however, are urged against the germ theory which are of high importance. The first difficulty may be stated thus: the spores or germs of definite fungi are themselves definite: they are not of infinite minuteness; these fungi nevertheless spring up when it is absolutely certain that their ordinary spores cannot be discovered. The second difficulty is this: the organisms found in putrescible solutions are of many varieties and species; there must be definite and distinct germs for each of these. You must, therefore, concede that the air contains multitudes of them, that every square inch of it is a magazine of innumerable varieties. The air would be then encumbered with germs. These questions may be styled those of the specificity and the plurality of germs.

According to the germ theory the essential cause of each variety of fermentation is a cellular fungoid organism. The objective phenomena of fermentation are but the results of the development, growth, and acts of life of the organisms. How, then, does this theory explain the apparently spontaneous fermentations, *i.e.*, when no determining cellule has been added to the organic solution? It is proved that a yeast cell can form in a saccharine fluid precisely as other varieties of organisms arise in other putrescible fluids. The fluid in which yeast cells float, and which contains no formed cells whatever, is exceedingly efficient in inducing saccharine fermentations. It follows either that those cells arise from molecules infinitely more minute than the cells themselves, or else the germ theory cannot stand. The careful researches of Dr. Beale have shown that molecules capable of development into perfect yeast cells are of extreme minuteness, many of them being much less than 1-100,000th of an inch in diameter. Such germs as these are readily capable of being wafted by the air and could defy the closest means of detection.

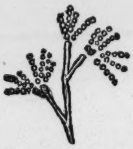
It was long ago ascertained that other fungoid spores than those peculiar to yeast could, in a manner precisely similar to the latter, induce fermentation.

Penicillium cultivated upon lemon gave rise to alcoholic fermentation, with the production of yeast globules, and, conversely, yeast globules were found to produce *Penicillium* as well as *Mycoderma.* It is obvious that this observation of the mutability of species in fungoid organisms is of the highest importance. The apparent structural characters of the spores and the subsequent developments and fructification of *Mycoderma cerevisiæ* differ widely from those of *Penicillium.* If they are thus mutually convertible, it would annihilate the argument advanced against the existence of atmospheric germs on the score that, to explain the different varieties of forms produced, one must concede the existence of an immense number of germs in the air of different kinds. This would tend to show, on the contrary, that the same germ under different conditions could assume the appearances and the functions of a different organism.

The most elaborate investigation of recent date bearing upon this subject has been made by Professor Hallier, of Jena.

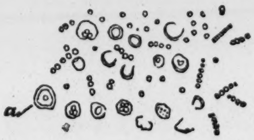
According to Hallier, the same germinal molecules develop according to the nature of the fermentescible substances in which they are deposited into the fungoid forms peculiar to each fermentation. The forms inducing putrefaction, fermentation, and mildew are all varieties of one another. As they develop within fluids they are cellular formations. When they grow upon the surface only do they present fructification (*Schimmel*). Hallier endorses Pasteur's view that the germs of all are carried by the air. The most abundant source of germs appears to be *Penicillium crustaceum* (see fig. 2), whose spores are universally spread because it is more hardy, more fertile, develops at lower temperatures, and grows and fructifies more rapidly than others of its kind. It will accomplish its growth and will bear fruit in forty-eight hours. A so-

FIG. 2.



Fructification of *Penicillium crustaceum.* (Hallier.)

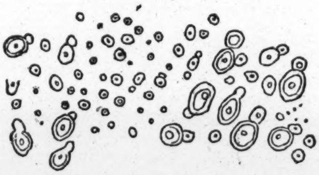
FIG. 3.



Spores of *Penicillium crustaceum* bursting in water and setting free their contained particles, which unite in rows or chains. (Hallier.)

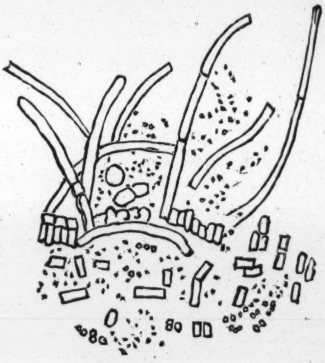
called spore of *Penicillium* falling into watery fluid bursts into a multitude of particles, each of which may be the radicle of a living fungus; the minute particles approximate and unite in twos, forming a double cell; moreover, they subdivide excessively rapidly, "so that the number produced can scarcely be expressed" (see fig. 3). The minute particles then unite in chains, constituting *Leptothrix,* which is not a species but a form of vegetation common to many species. In pure water development can go no farther, nor after a few hours do the organisms continue to be formed; for further development the presence of a nitrogenous substance is necessary. The minute spherules (*micrococci*) are the special ferments of putrefaction; in the presence of sugar the spherule enlarges and becomes a nucleated cell (*cryptococcus*) identical with the yeast-cell (see fig. 4).

FIG. 4.



Cryptococcus in various grades of development from *Penicillium.* (Hallier.)

FIG. 5.



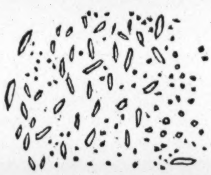
Milk ferment.

A very similar change takes place in oil fermentation. In milk the *micrococcus* elongates and forms parallelopipeds or staff-like cells (see fig. 5); in acetic fermentation the cells assume a lancet shape (see figs. 6 and 7). These ferments increase by division, and are classed by Hallier under the term *arthrococcus.* If we can accept the teaching of the foregoing evidences, fermentation and putrefaction are both due to the influence of a single agent —vitalised matter—which is transported from place to place by the air. We must believe that ordinary air contains minute masses of living matter—call these particles germs, germinal matter, protoplasm, bioplasm, or what we will—that they are derived from the fructification of fungi which can spring up wherever nitrogenous organic matter is in contact with air; that from such fructification it is not the visible spores but minute fragments of these which are the first causes of the subsequent decompositions; that these living molecules grow wherever they find a soil meet for them, and in different soils develop into different forms, and produce by their vital acts different effects.

It now appears that none of the objections hitherto urged are fatal to the germ theory. Chief, however, amongst the arguments adduced against it has been that deduced from the results of destructive agencies.

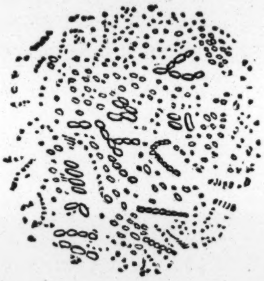
At a very early period in the history of the controversy concerning the origin of life, investigations were made with the intention of determining this question. Can vital organisms be found to develop under physical influences which are, under ordinary circumstances, fatal to any living thing? If so, there is a great difficulty in the way of accepting the germ theory. The heterogenists have argued that living bodies *are* discovered after processes of physical destruction which it is *impossible* for life-possessing matter to withstand; therefore, by exclusion, spontaneous generation must occur.

FIG. 6.



Acetic ferment in course of development. (Hallier.)

FIG. 7.



Acetic ferment from the surface of stale beer.

The necessary conditions for experiments of this nature are—(1) the subjection of fermentescible matter to such destructive agencies that vitality in it is impossible; (2) the prevention of any subsequent contact with it of an agent which may be a possible vehicle of vitality.

The agency usually employed for the destruction of all possible life in fermentescible fluids has been heat; the precautions against the admission of possible germs or germ-laden air have been various. The most simple of these processes has been the subjection of the fermentescible fluid to heat, varying in intensity in different experiments, and its preservation in sealed flasks whence air was as rigidly as possible excluded. The originator of this method of investigation was Needham. Since his time it has been put in practice with varying methods for the exclusion of possible germs after the heating of the fluid. The temperatures employed have varied in different experiments from boiling for periods ranging from a few minutes to six hours (Pouchet, Mantegazza, Child, Bastian) to 153° C., or 307·4° F. (Bastian). The means of excluding possible vitality subsequently have been passing the air through a tube raised to a red or white heat (Pouchet, Wyman, and others), or through a tube containing strong sulphuric acid (Pouchet, Joly, Musset), or supplying only oxygen (Pouchet, Child, Mantegazza), or artificial air (Pouchet, Child), or by preserving the boiled putrescible fluid *in vacuo* (Pouchet, Bastian). In all these cases organisms have been described as resulting. In these experiments it would appear that the conditions would be fulfilled with the least liability to objection in the cases wherein the solutions were exposed to very high temperatures *in vacuo*. M. Pouchet filled a flask with a decoction of malt which had boiled for six hours, and hermetically sealed it. After six days a deposit, apparently of yeast, took place in the flask; on the seventh day the external temperature having been suddenly raised the flask burst with a loud report. There had been fermentation, and the microscope disclosed that yeast corpuscles had been formed. Dr. Bastian has also performed analogous experiments with great care. In his first series, the fluids were boiled in small flasks for ten to twenty minutes, and then by means of the blowpipe hermetically sealed; after periods varying in the different experiments from five to thirty-nine days, the flasks were opened, and the contents were examined with the result of the discovery of many lowly-endowed organisms. Living bodies were also found when solutions of tartrate and phosphate of soda and some other saline solutions were in like manner made the subject of experiment. It has been supposed that these observations are very antagonistic to the germ theory, and lend colour to the views of heterogenists. In briefly considering this conclusion we may divide the arguments into two classes, the first being derived from the evidence of the supposed destructive influence of high temperatures on living matter, the second from the supposed impossibility of the pre-existence of germs in the fluid employed. It must be recollected that by the argumentation in favour of heterogeny the method of exclusion is employed, and this requires the most prolonged and patient employment before its conclusions can be said to approach finality. Furthermore, its upholders must be prepared to substitute for the explanation of phenomena which may be derived from any other theory another which shall be more satisfactory.

Let us, in the first place, accept as proved, by the results of the experiments, that living forms have been discovered after the subjection of the fluid containing them to a temperature of 307·4° F.

The affirmative proposition necessary to establish the heterogeneous evolution of these organisms is that no living thing can in any state and under any conditions withstand a temperature of 307·4° F., without annihilation of its vitality.

It is probable that this would, according to their preconceived notions of the properties of living bodies, be at first sight adopted by most people.

It is difficult, *à priori,* to believe that living matter could resist such a seemingly powerful cause of destruction; but it behoves us to consider the subject, not solely according to our preconceived ideas, but to inquire as to the properties and the behaviour of living matter from observation below the surface of ordinarily visible nature. And here we find many seeming paradoxes: we find that it possesses powers of persistence and of resistance, which would certainly, *à priori*, appear impossible.

First, as to persistence. We know that vitality may lie dormant for a period which is almost inconceivable. Stramonium seeds, according to Duhamel, can develop after remaining twenty-five years under ground. Friewald observed the germination of melon seeds after they had been kept more than forty years. Pliny asserted that corn grew after it had been kept 100 years. Desmoulins obtained plants from seeds found in a Roman tomb of the third or fourth century. Finally, it is well known that corn found in some of the tombs of ancient Egypt has germinated and grown to perfection, and that a squill-bulb, found in the hands of a mummy has, when planted in this age, and in this country, grown and blossomed. In these cases it can scarcely be questioned that, remarkable as it may seem, the vitality (or term it what we will) of the various germs has slumbered during the protracted periods indicated.

The proposition that there have been in these cases actual death and subsequent reviviscence cannot be seriously sustained, least of all by those who uphold the heterogeneous evolution of living things according to definite and progressive stages by which the identical original form could only by a miracle be obtained. Concerning lowly-endowed organisms, Claude Bernard taught that "infusoria carefully dried lose all vital property, at least in appearance, and can remain thus for whole years; but when water is restored to them they re-commence their life in the same manner as formerly, provided a certain degree has not been overpassed in the desiccation."

These facts alone are sufficient to show that we must rely on no *primâ facie* or surface ideas for our conception of the nature and properties of those bodies which possess vitality.

Then, as regards resistance on the part of living organisms, especially with regard to temperature.

First of all, we have *direct* evidence as to the influence of temperatures on low organisms. We have abundant evidence that they can support very low temperatures, very high temperatures, and very rapid alternations of temperature. Bacteria and monads survive a cold of 23° F. for an hour, and often 5° F. for a few minutes. The lowest organisms "possess for the most part," says an ardent supporter of heterogeny, "a resistance often surprising to heat and cold." An experiment of M. Pouchet shows even that certain of them can easily support sudden changes of temperature, even a rapid transition of 100° C. In the hot Geyser springs which reach nearly to the boiling temperature, unicellular plants have been found growing. Practically, however, the extreme limit of heat which it is found that *developed* organisms can bear in the presence of water has been fixed at 100° C., *i.e.*, the temperature of boiling water, and this by the upholders of either side. In dry air, organisms are capable of withstanding a considerably higher temperature than when they are contained in fluids.

In the ovum and spore condition, life-possessing matter, according to all the evidence, possesses a higher power of resistance than obtains in case of the developed organism. The zoosperms of the frog can retain their vitality in a cold not exceeding 24° below freezing. M. Payen determined that the sporules of the *oidium aurantiacum* resisted a moist heat of 248° F., and only lost their faculty of germination at a heat of 284° F. Pasteur asserted that spores of mildew *in vacuo* or dry air were fertile after exposure for twenty minutes to half an hour to a temperature of from 248° F. to 257° F., but he concluded that exposure for more than twenty minutes to 260° F. to 266° F. completely destroyed their vitality. It is thus seen that organised material in the embryonic condition is capable of resisting temperatures which seem at first sight almost impossible. An argument to the contrary has, however, been urged from the behaviour of known and recognised spores under such conditions. Pouchet observed that the spores of *Ascophora*, *Penicillium*, and *Aspergillus* were completely *disorganised* by being boiled for a short time in water. So, also, Bastian showed that a fungus and spores heated in a sealed flask for four hours to 153° C.; that is to say, treated in precisely the same manner as the solutions which yielded him, in the case of infusions, evidence of life and organisation, were completely disorganised. "Not a single entire spore could be found; they were all broken up into small, more or less irregular, particles." But it behoves us to inquire further as to the signification of this disruption. We can quite agree that by the influence of the heat the spore is torn asunder and dissipated into fragments, but it is another thing to assert that such fragments are bereft of all vital property. Observations have all tended greatly to show that our notions concerning a "spore" must be modified. It is not, like the seed of a phanerogamous plant, the nucleus of a single organism, but a collection of extremely minute individual particles, each of which may become a definite organism. Though, therefore, we may agree that there is an apparent disintegration of the visible spores, we need not subscribe to the view that every one of the individual particles succumbed to the destructive influence. Disruption need in no way connote destruction. The divided polyp is not destroyed, but its fragments grow into fresh organisms, and a dismembered portion of a plant can become an individual tree. We may agree that the spores have lost the power of reproducing the plant whence they were originally derived, but we know also that the surroundings and the conditions of pabulum are greatly changed by the influence of the heat, and this may be a sufficient explanation. One observes when mildew grows upon organic matter, that though a certain species (say *Aspergillus*, for instance) may be shedding its spores in all directions, these do not spring up as successive crops of *Aspergillus*—a species totally distinct in form succeeds it, and so on through the generations following. We are not bound to believe, therefore, from the apparent evidences of physical destruction, that every particle is rendered lifeless; certainly we cannot conclude this from *à priori* grounds. To say that because higher forms would have lost vitality, therefore lower or embryonic forms must lose it likewise, would be equivalent to estimating the power of resistance to physical influences of a spermatozoon from the power of resistance of a developed animal.

Dr. Bastian asserts that the vitality of vibrios and bacteria is destroyed by the boiling temperature: when infusions containing active bacteria and vibrios are boiled, the result is the disruption of the vibrios and the disappearance of all signs of life in the bacteria. "All their peculiarly vital acts have at once ceased, and they have henceforth displayed nothing but mere Brownian movements." It must, however, be remembered that such diagnosis of vitality is purely arbitrary. In the course of putrescence, no one can tell when movements cease to be Brownian and commence to be vital; conversely, when the special bacterian movements have been caused by heat to cease, no one could assert that the movements of the *débris* were purely mechanical; or, if all movements had ceased, it could not be positively stated whether vitality had been annihilated or only paralysed.

In face of all the facts it cannot be said that the heterogenists have proved their case, that subjection to a heat of 153° C. is an absolute test of the absence of vitality. When we see the extraordinary powers of persistence and of resistance of life-possessing matter, any single test which we may impose, unaccompanied by collateral evidence, cannot satisfactorily prove the absence of vitality. The very experiments themselves, which are supposed to prove the impossibility of vitality by the stringency of the adverse influences employed, demonstrate how vital matter defies such adverse influences. Could one predict that, even if low forms could originate, complete fungi could grow and fructify in the conditions of vacuum and of pabulum which would be profoundly altered by the exceedingly high temperature? But not only so; in some of Dr. Bastian's experiments the growth of organisms seems to have been *favoured* by the conditions. As, therefore, with phanerogamous plants there is a wide range of temperature-conditions most befitting the perfection of different species, so there is no reason for denying to lower forms a wider range than our *à priori* views would have led us to imagine.

We will now turn to the second part of the argument advanced in favour of the heterogeneous evolution of the organisms found in these conditions, viz., the impossibility of the pre-existence of living matter in the materials employed in the experiments. The chief evidence in this direction is adduced by Dr. Bastian from his experiments with saline substances. In examining crystals of the neutral tartrate of ammonia, Dr. Bastian found in their interior positive evidences of fungoid germs. Far from this being an argument in favour of the transformation of crystalline into living matter, many will consider that it lends weight to the germ theory. Upholders of the latter assert the universal presence of germinal molecules. Dr. Bastian shows them to exist in crystals by physical demonstration. That once in a saline solution such vital organisms can grow and propagate is no matter of doubt—it is admitted by either side. Pasteur showed that a salt of ammonia and the phosphates could become a perfect pabulum for torula cells. The oxalates and phosphates have been shown by Hallier to be excellent media for the development of fungoid fibres, oidium forms, mycoderms, and fructifying organisms. The whole difficulty rests, therefore, with the earliest phenomena.

It behoves us to inquire into the teaching of the negative as well as the positive results. That the proliferation of forms of life under restrictive influences such as have been mentioned is exceptional is taught by the results of every labourer in the field. Privation of air is admitted to be a most potent cause in prevention of the appearance of organisms: or to put it in another way, "experiments in closed vessels are quite unfavourable to the demonstration of heterogeny, because the natural and regular progress of the phenomena is paralysed." Increment of heat is another adverse influence, "because the destructive agent impairs without destroying that organising force which is an essential property of organic matter." These are the modes of expressing the facts adopted by the heterogenists; and MM. Joly and Musset, by actual experiment, prove that the organisms in a putrescible solution submitted to ebullition "are more simple and less numerous accordingly as the ebullition is prolonged." In fact, we must conclude with M. Pennetier "that the phenomena of spontaneous heterogenic generation, intense while their regular course is respected, are nevertheless manifest, though in successively lessening degrees, as causes of difficulty are increased, to finally cease when the phenomena of fermentation and putrefaction are themselves prevented." This as an *explanation* of the position to which science has arrived in this question it is scarcely needful to say is feeble in the extreme; it is merely a categorical expression of results which can be explained at least as well by the one theory as by the other.

From the foregoing data it appears that none of the arguments adduced are sufficient to invalidate the germ theory. There is a mode of putting the question which must appear to many to be unfair. It is alleged that the onus of proof rests entirely with the Panspermatists. It is not for the heterogenists to prove that germs do not exist, but for the Panspermatists to prove that they do exist. "The charge of proof in science rests with those who allege a fact." Surely the facts support precisely the contrary view.

The positive proposition '*omne vivum e vivo*' is asserted by all the evidence of visible nature, and by microscopic research, to those confines beyond which human powers cannot reach. That there is an exception to the seemingly universal law in the case of those organisms which are invisible it must be the duty of those who embrace the theory of spontaneous generation to prove, or else to tolerate becomingly the scepticism of others. It behoves us seriously to weigh the only real objection to the reception of the germ theory—the resistance to the destructive agency of heat, and to inquire as to the effects of other physical agencies which may contribute to a solution of the question. It is not by the results of a single method of investigation that this question is to be judged, but rather by the collective evidence of many methods.

Heat is not the only destructive agency which may be employed in the inquiry: others, fraught with much valuable teaching, may be put in force, though these have been apparently in the recent controversies entirely ignored. Such are the evidences derived from the destructive influences of chemical and of poisonous agents. It has been known from time immemorial that the addition of certain compounds prevents both putrefaction and fermentation. The belief being that these processes were essentially chemical, it was naturally probable that the agents which suppressed them should be susceptible of a chemical classification: but the infinite variety and opposite properties of the various agents precluded this classification. If the processes were, as asserted, those of oxidation, it would surely be not unreasonable to expect that the agents which arrest them should also arrest oxidation; but common experience taught an absolutely contrary lesson—that *oxidising* agents were the most efficient in arresting the processes. Again, on the chemical theory there ought to be some quantitative relation between the amount of a chemical agent employed and the degree of its influence; but the fact is that an agent present in such feeble quantity as to be capable of no appreciable chemical effect on a mass of putrescible material is yet capable of stopping all putrefaction. Furthermore, agents, such as carbolic acid, which are proved to exert no influence whatever on processes purely chemical, are among the most efficient of all means for preventing putrefaction and fermentation.

A large series of observations shows, on the other hand, that the agents arresting these processes exert their influence precisely in so far as they are *poisonous* agents to low organisms. If at any stage of the process these microscopic organisms are rendered lifeless, the process, with all its attendant phenomena, ceases: on the other hand, the overt signs grow with their growth, strengthen with their strength, subside when they languish, and cease when they die.

Hitherto experiments have usually been made with the view of ascertaining the effects of antiseptic agents when mixed with putrescible material; but the author has attempted to ascertain the results which occur when the air alone is influenced by certain agents; the materials being left intact. In this way the evidence of the results obtained from experiments with *heat* may be tested by the evidence of other agents of vital destruction.*

The results obtained may be thus briefly summarised:—

1. Putrefaction, mildew-formation, and the appearance of organisms can be checked or absolutely prevented by the existence of certain agents in the air supplied to a putrescible body.

2. The power of such agents can in no sense be measured by their chemical constitution or characters. From many experiments the following expresses their order of efficiency from weakest to strongest:—(1) chloride of lime; (2) sulphurous acid, ammonia, sulphuric ether; (3) chloroform; (4) camphor; (5) iodine, phosphorus, creosote, carbolic acid.

3. The agents which stop fermentation are vegetable, not animal, poisons. Fungi will grow in the presence of hydrocyanic acid and of strychnia.

4. Comparative experiments show that a given volatile agent is far more efficient when it is contained in the air supplied to a putrescible solution than when an equal quantity is mixed with the solution itself.

(5) All fungoid organisms can be prevented by the presence of a minute proportion of creosote, carbolic acid, ammonia, hydrochloric acid, or sulphurous acid in the air, though beneath the surface of the fluid are found numerous bacteria and vibrios.

There seems to be no escape from the conclusion that the germs of fungi exist in the air and are destroyed by the volatile poisonous agent.

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* See paper by the Author in the "Chemical News" (vol. xxii, pp. 241, 254), "Evidence concerning the Germ Theory of Fermentation afforded by the Action of certain substances when Suspended in the Air."

III. MOLECULES, ULTIMATES, ATOMS, AND WAVES.

By Mungo Ponton, F.R.S.E.

Part I.

CHEMISTS have, for a considerable number of years, recognised the convenience of distinguishing the particles of chemical compounds by the term "molecules," reserving the term "atoms" for those of the chemical elements; but the time seems now to have arrived when it has become needful to make a further discrimination. This need has been rendered manifest by the results of spectrum analysis. The word "atom" conveys the idea of a particle incapable of being analysed; but the spectrum has shown the particles of the chemical elements to be otherwise constituted. Had they been simple homogeneous masses of definite size and weight, each element, when thrown into vapour and rendered incandescent, would have exhibited in the spectrum only a single bright line; because every particle of the element being precisely alike would, when in the vapourous state, and freed from all extraneous influences, have vibrated in exactly the same periods of time, and so have originated luminous waves of only one definite length and period. The discoveries of Messrs. Bunsen and Kirchhoff, however, and of other labourers in the same field of research, have shown that the chemical elements, when in the state of glowing vapour or gas, exhibit more than one bright spectral line—some of them, indeed, a large number of distinct lines of various degrees of brightness, in widely different regions of the spectrum. Even hydrogen, the element which is at once the lowest in specific gravity, and the lightest in the scale of chemical equivalents, exhibits four bright spectral lines, while iron presents a large number.

The law of chemical combination by equivalent weights appears to exclude the supposition that the ultimate particles of any element differ in size and weight—the equivalent being merely an average. For the smallest quantity of the vapour of any chemical element shows the same number and kind of lines as any greater quantity; while there is an extreme improbability that this should be the case, did the ultimate particles differ in size and weight, and were the combining proportion merely a general average of those diverse weights. The only alternative conclusion appears to be, that each ultimate particle of the element consists of numerous more minute atoms, differing in their inertia, and held together by a force too great to be overcome by any chemical means which can be brought to bear upon them for the purpose of effecting their separation. This view at least possesses such an amount of probability, that it may be fairly assumed as a basis of argument and of further investigation.

Making this assumption, then, it will be found convenient to designate the ultimate particles of the chemical elements by the term "ultimates," reserving the term "atoms" for their assumed constituents. We shall thus have the three terms, "molecules," "ultimates," and "atoms;" the first denoting the particles of chemical compounds, the second those of the chemical elements, and the third the assumed constituents of those ultimates, the atoms being themselves incapable of further analysis. The term "particle" may be applied generically to embrace all the three.

So far as observation goes, the only way in which vibrations can be excited in the infinite luminiferous ether is by corresponding vibrations in the particles of ponderable matter. On the other hand, these latter may always be excited by pre-existing vibrations in the ether itself—the one set of vibrations acting and reacting on the other. We are ignorant, however, of the manner in which vibrations may be excited in the particles of ponderable matter, apart from any pre-existing vibrations in the ether, or from the conversion of progressive into vibratory motion by collision. Electricity is ostensibly one kind of force which may be available for this purpose; but then we know not to what extent the ether associated with the ponderable particles may be needful to the existence of the electricity, although we do know that it can neither exist in the free ether, nor pass through it without the help of ponderable matter. Waiving this question, let it be assumed that the vibrations in the particles of ponderable matter are excited by some adequate force—some kind of motive energy.

The amount of vibratory motion assumed by the particles will depend—(1st) on the degree of motive energy applied, and (2nd) on the inertia of the particles—their resistance to the applied force. This inertia is of two sorts, (1st) intrinsic, (2nd) adventitious. The intrinsic is that possessed by each particle in itself, independently of its relations to other particles with which it may be associated. The adventitious inertia arises out of those relations. It is the immobility which any particle possesses in virtue of the forces exerted on it by other neighbouring particles. The intrinsic inertia predominates in gases and vapours, the adventitious in solids and liquids.

Incandescence begins in all solids and liquids at precisely the same temperature. A red heat or a white heat indicates the same temperatures whatever be the nature of the incandescent substance, provided it be either in the solid or liquid condition. But then it requires very different amounts of applied motive energy to raise different substances to the temperatures indicated by those phenomena of incandescence. This amount depends on the specific heat of the substance, or the inertia of its particles. Thus the motive energy required to raise water to the temperature of a red heat is about 33 times the amount requisite to raise an equal weight of mercury to the same temperature; yet both substances would exhibit the same tint of redness. Part of this difference may be due to the greater space over which the motive energy is diffused in the case of the water, which has upwards of 13 times the bulk of an equal weight of mercury; but for equal volumes the water still requires about two and a half times more applied energy than does the mercury. Both substances being in the liquid condition, this difference can be most readily explained on the supposition that the molecules of water have a greater intrinsic inertia than have the ultimates of mercury.

It is the adventitious inertia, however, that is most prominently displayed in the case of incandescent solids and liquids. The difference in the colour of the light which they exhibit is an exact measure of the degree of rapidity of the vibrations communicated to the luminiferous ether by those of the ponderable particles. The two sets of vibrations must be synchronous; and as we can determine the rate of the ethereal vibrations appertaining to each tint, so we can ascertain the rate of the corresponding vibration in the particle by which the ethereal vibration is generated.

In all solids and liquids the force of cohesion by which the particles are held together must act on them very unequally, according to their position in the mass. Those at and near the surface are almost entirely freed from the operation of that force in one direction, while on those within the mass the cohesive attraction acts in all directions. The latter must, therefore, have by virtue of their position a greater amount of immobility than the former, and they will remain at rest after the former have been set in motion by the applied force, or at least they will more slowly acquire their proper rate of vibration. When incandescence begins, it is only the most movable particles—those at and near the surface—that vibrate with sufficient energy to generate luminous vibrations in the ether. They are driven to a comparatively considerable distance from their points of rest, and the force by which they are dragged back again being comparatively weak, they take a longer time to perform their excursion. They accordingly generate only red waves in the ether. The light is feeble, and the spectrum it exhibits is chiefly confined to the region of the fixed lines A and B. As the motive energy increases, those particles situated a little farther in from the surface begin to vibrate at their proper rates. Their excursions are performed in a more limited space. The force by which they are dragged back to their points of rest is greater, consequently their periods of vibration are quicker. These give rise to orange and yellow waves in the ether, and the spectrum gradually extends towards the line D. A still greater energy sets more of the particles into violent motion. These perform still quicker vibrations, in virtue of their greater adventitious inertia. At last the light becomes quite white, and the spectrum extends continuously from the extreme red to the extreme violet. The particles of the substance are then vibrating at every rate of rapidity embraced within the limits of the visible spectrum, and also at various other rates both slower and quicker than these. This variety of rate cannot be due to diversities in the intrinsic inertia of the particles themselves, and can be explained only by differences in their adventitious inertia, arising from their position in the mass and the various degrees in which they are influenced by the force of cohesion.

When a substance is thrown into vapour, the operation of the cohesive force is suspended, and the molecules or ultimates are held together merely by the force of gravity. They are free to perform larger excursions; consequently, while the amplitude of their vibrations is increased, their rapidity is so much lowered that they cease to propagate through the ether vibrations of those rates which fall within the limits of the visible spectrum. It is not till the applied motive energy becomes greatly augmented that a vapour begins to propagate luminous vibrations, and these are then found to be no longer capable of exhibiting a continuous spectrum, but only definite bright lines—showing that there is an entire alteration in the mode of their production. The same remarks apply to permanent gases.

In some few instances incandescent elemental vapours exhibit, in addition to their characteristic bright lines, a diffuse, weak, more or less continuous spectrum. The most probable explanation of this phenomenon is an imperfection in the vapourisation. Minute incandescent solid or liquid particles may be carried up in intermixture with the vapour, and give rise to a weak continuous spectrum. At least it would not be easy to find any other more feasible explanation.

Confining attention to the definite bright lines, and assuming these to be due to the vibration of the atoms constituting the chemical ultimates of the gas or vapour, it is plain that these atoms, while very close to each other within the limits of the ultimate, cannot be in absolute contact, otherwise they would be incapable of separate individual vibration. They must be held together by a very powerful atomic attraction; but they must be simultaneously kept apart by a correspondingly powerful elastic or repulsive force which prevents their passing into absolute contact. This elasticity can be no other than that of the luminiferous ether, which, probably in a compressed state, must intervene between the atoms and keep them asunder, balancing the atomic force at a certain minute distance.

The nearest analogy to such a condition is that furnished by the extreme points of two similarly magnetised fine sewing needles. Suppose the points to be both north poles, and one of the needles to be fixed in an upright position with its point uppermost, while the other is suspended over it with its point downwards. In the absence of the magnetism, the suspended needle would, by the force of gravity acting alone, be attracted into a position exactly over the other, so that the two needles would form a straight vertical line. But the magnetic repulsion subsisting between the two points will prevent their assuming this position. The upper needle will become slightly inclined to the other, the two points being maintained at a certain distance apart, depending on the strength of the magnetism. In this case, the magnetic repulsion acts the part of the elastic ether, while the terrestrial gravitation performs the office of the atomic attraction in the other case. But the atomic attraction must in its intensity approach very nearly to that of the repulsion exerted by the ether, so that the two forces may balance each other. Were the repulsion exerted by the ether much in excess of the mutual attraction of the atoms, these could not approach so near one to another as they do. Were the attraction in excess of the repulsion, the latter could not prevent the atoms from passing into absolute mutual contact.

If such be the constitution of the chemical ultimates, it may not be improbable that, in those cases in which the bright spectral lines are very numerous, part of the effect may be due to the various degrees in which the atoms composing the ultimate are influenced by the atomic attraction, instead of its being wholly due to differences in the intrinsic inertia of the atoms. The diverse rates of vibration may be partly owing to mere differences in the position of the atoms within the limits of the ultimate, as in the case of incandescent solids and liquids. Among atoms having the same intrinsic inertia, those at and near the surface of the ultimate will have slower rates than those in the interior. Indeed, each ultimate may be regarded as a minute incandescent mass, whose atoms are vibrating at different rates—only the rates are much less various than in the case of solids and liquids; so that only vibrations of certain definite rates are communicated by the atoms to the ether—giving rise to the definite spectral lines. Doubtless, however, where these lines are few in number, differences in the intrinsic inertia of the atoms themselves are probably more efficient in causing diversities in the rates of vibration, than mere differences in the position of the atoms relatively to each other, within the limits of the ultimate which they unite to constitute.

When the elementary gases or vapours act as absorbents on white light passing through them, they produce in the continuous spectrum of the transmitted light dark lines, exactly corresponding in position to the bright lines which they themselves generate. This effect cannot be explained otherwise than by the transference of the vibratory motion passing through the ether to the particles of the gas or vapour. But were it transferred to the chemical ultimates in their integrity, they being all of the same weight in the same element, could take up only the vibrations performed at one single rate or some octave of that rate. The circumstance, however, that they absorb definite lines in diverse and distinct parts of the spectrum, shows that the motion must be taken up by different atoms having in virtue of their intrinsic inertia a tendency to vibrate at those different definite rates. For it seems highly improbable that one and the same individual atom, or that atoms all exactly alike in their intrinsic inertia, could select and assume different definite rates of vibration from among the vast number of rates presented in the continuous spectrum.

The appearance of the lines, whether bright or dark, is much influenced by the temperature and pressure to which the gas or vapour is subjected. Increase of temperature augments the brightness or blackness of the lines, and in some cases adds to their number, but it does not affect the breadth of the individual lines. Where it adds to their number, the new lines are always developed towards the violet end of the spectrum. They correspond to quicker rates of vibration, the same order being followed as in the case of incandescent solids and liquids. A diminution of pressure narrows and weakens the lines, sometimes extinguishing them altogether. An increase of pressure brightens or blackens the lines, and adds considerably to their breadth. When the increase of pressure becomes very great, the breadth of the bright lines becomes so augmented that they ultimately blend and form a continuous spectrum, as if the substance had become liquid or solid, although it nevertheless continues in the gaseous condition. This last phenomenon indicates a development of adventitious inertia, in virtue of which the atoms that, under a more moderate pressure, vibrated at only certain definite rates, are, under the influence of the augmented pressure, compelled to vibrate at various rates, and behave like the particles of solid and liquid bodies. The most obvious explanation of this fact is, that the chemical ultimates are really very complex in their structure. While they consist of atoms having different degrees of intrinsic inertia, in virtue of which they tend, when freed from all adventitious inertia, to vibrate at only certain specific rates, nevertheless even the lightest ultimates consist of a considerable number of such atoms; so that, when they are forced into greater mutual proximity by strong pressure, they act and react on each other by their atomic attractions in such a manner as to develop within the limits of the ultimate an inertia of position, in virtue of which the constituent atoms vibrate at a much greater variety of rates.

IV. THE GREAT PYRAMID IN EGYPT,

FROM A MODERN SCIENTIFIC POINT OF VIEW.

By C. Piazzi Smyth, Astronomer Royal for Scotland.

(Continued from page 35.)

Part II.

Collecting Observations on the Present, from which to Deduce the Past, Phenomena of Appearance and Fact.

1. *Principles of Observing.*

WHAT a relief is it always to the over-strained soul of man,—after being driven to and fro between the storms and counterblasts of opposing theories of causation, on subjects, too, where mere theories of the present day can avail but little,—when the appeal to observation begins! That is, at least, if the appeal be conducted more or less, and *mutatis mutandis*, on the same observing principles which are now so well followed out in most astronomical observatories.

I mention *them* so prominently here, because nowhere else, perhaps, among all the sciences, is the arena of pure and direct observation so far removed from that of theoretical investigation, in all that concerns its time, place, manner, means, and often the men to be employed. On all which accounts, as well as for the never ending multiplicity of things to be observed, and the readiness with which the *apparent* phenomena lend themselves as so many convenient hooks whereon the observer may at once hang and duly arrange all the numerical measures that he can ever make, a different world of thought and exertion is opened up for a time to the earnest student.

A world, too, where such person may safely let every regard for the ultimate conclusions which may be educed from all his present work remain in perfect rest, while he attends, not only to procuring the data of observation, but indelibly recording them also; with full accompaniments, too, as to their limits of probable error, from whatever cause arising. Above everything, moreover, endeavouring to free himself from past prejudices dependent on mere opinion; while seeking rather to cultivate a teachable, inquiring, learning frame of mind, as well as sufficient of the spirit of humility to perceive, that though we may be able to stand so easily on the dead of ages ago, it is a *lion* dead that we are standing on in the Great Pyramid case, and through no superior innate virtue of our own.

All this may haply form but a dull occupation to many minds; yet is it a positive duty to be performed. And if I spent five *months* in carrying it out to the best of my ability in Egypt, my readers will hardly complain if five *minutes* of their time be now occupied in hearing little more than details of numerical measures. Fully convinced, too, as they should be in their own minds, that such work is an absolutely necessary *Red Sea* of difficulty, toil, and trouble which all must wade through if they would fain reach eventually the promised land of sound theory, and thence behold some glorious rays of primeval truth concerning man, and even haply ascertain *why* these early things which we behold were *so* made and fashioned.

2. *Of the Things to be Observed.*

After the traveller has enjoyed his first and very distant view of the Pyramids of Jeezeh, from the deck of his Nile boat, or in modern times from the window of a railway carriage, as both the one and the other conveyance are nearing Cairo, and are passing a particular neck of flat, open land, just where the grand African river, which has rolled its floods from the South through so many thousands of miles of a single continuous channel, suddenly parts them into a fan-like form of many branches, producing thereby the Delta shaped land of Lower Egypt; the said traveller, I say, is rather bewildered than enlightened, and astounded more with confusion than sublimity when he *afterwards* of set purpose reaches the pyramids themselves, and actually treads the ground on which they stand.

In the distant view, both the great and the second great, and sometimes even the third great pyramid, stand up sharp and clear on the horizon line, with their triangular sides glittering towards the sun like the facets of a regular crystal, and you could then fancy any amount of refinement and finish of their surface; while the country between shows you only calm reaches of blue water, and wide expanses of greenest corn-land, with the occasional waving fronds of a gracious palm-tree, crowning a village knoll or adorning a garden grove in the nearer distance that intervenes.

But, on reaching the very pyramids by the usual road from Cairo, you have already turned your back on everything that is green throughout all the land of Egypt; the silent, and white and yellow desert is about you on every side; and those stupendous monuments of early man seem, if anything more than mouldering heaps of rudest stones, to have been built somewhat in cyclopean sport, as flights of audacious, mocking steps, some three to four feet high, each of them.

To describe these appearances fully, and, for their own sake alone, would be merely to deal in the poetic, or picturesque, and such telling effects as ruin and decay invariably import into the architectural works of man. But this sort of thing has been done often enough before; unfortunately *proves* nothing; and is not our true business here. We have come, if for anything, to search for traces of the *original* forms before all this dilapidation began. Such traces, too, there are; many, perhaps, still entombed in the rubbish of latter ages, and only to be opened up to the attentions of the scientific measurer by extensive excavations, too costly for *him* to undertake; but numerous are the traces of exactness still remaining above ground, if men would only seek them out earnestly. This, however, a few persons *have* of late begun to do; whence it has already arrived, that our knowledge of the original condition of the pyramid is much more advanced now than it was only a few years ago.

Every one knows the usual mathematical definition of a pyramid, viz., a solid whose base is a regular rectilinear plane figure, and whose sides are plane triangles, having all their vertices meeting together in a point above the base, called the *vertex* of the pyramid."

Give, then, to the base of such a conception *four*, rather than any other number of sides, and you have at once (with only a possible variation in the angle of slope of said sides) the very figure which you saw so bright, so clear, so inimitably defined, so blocked out in the solid, and yet so delicately brought up to a summit point, in the distant view. But now that you are close to it, on the hill of Jeezeh, the scientific question that arises is, to what degree of accuracy was the practical realisation of such theoretical idea really carried out, when on the vast scale of the Great Pyramid? And our answer must evidently depend, not on the general ruined heaps lying about, but on those exact traces of the original which, as I have said, *are* to be found in no inconsiderable number by those who seek for them aright.

3. *Situation of the Great Pyramid.*

Perhaps the first point in the general problem to which we should direct our attention is that of location or situation.

I have already, in Part I., set forth that the Great

Pyramid is not founded clumsily and boorishly on mere alluvial mud low down in the Nile valley, as taught by some, but is raised on the top of a broad and lasting hill of nummulitic limestone, more than a hundred feet above the level of either the Egyptian valley or the highest range of its river's floods; or more exactly, for a purpose which will afterwards appear, 1780 inches above the well-water level, and 2580 inches (nearly) above the Mediterranean Sea-level. But there is a notable manner about its emplacement even there, indicating forethought and preparation on the vastest scale; well illustrated, too, by a spontaneous and irrepressible remark of Mr. Aiton (professionally a railway-cutting contractor) when he visited my wife and self, then living on the Jeezeh hill; for he would insist, in telling workman language, on the patent evidence to *him*, after walking about every side of the Great Pyramid, that all the ground around it was "excavated ground."

Such was his technical phrase for describing what had previously struck me, viz., that the whole of the original top of the hill was artificially removed, or had in a manner been sliced away horizontally, slice after slice, until at length an admirable quality of dense, sound rock had been everywhere obtained, on which to lay the foundations of the new building with security, and to form a level area of lasting character round about it.

Within the intended compass indeed of the building itself, certain portions of the living rock had been left standing, so as to save masonry, but were only so left when of good quality, and after having been *cut* into regular horizontal steps suitably to joining on with the courses of the masonry. Even the topmost surfaces, therefore, of these internal rock portions of the pyramid, are not of the original surface material of the top of the hill before the pyramid builders began their work.

What was that original surface then?

There is a hill about a mile to the south which answers the question admirably. Its middle height is composed of horizontal limestone strata, whose transverse sections, one over the other, form a cliff towards the north; a cliff, too, so solid, so regular in its constructive layers, that one has to look long and attentively to be quite sure that it is not a vast Cyclopean wall of art and man's design. But no! it is Nature's own building, the edge only of one of her vast platforms, whose surface in this case is covered and hidden by the top of the hill, and that top is angular, rough, ragged and horribly rotten, with a geologically later limestone belonging to the tertiary period.

That then, as the near approach to horizontality in all the geological strata hereabouts shows, must have been the sort of stuff which the Great Pyramid builders had to clear away in vast quantities from the top of *their* hill also. Yet, truly vast as must have been the amount of it then removed—"excavated" Mr. Aiton would say—it was cleared away by them so thoroughly that neither Strabo, Diodorus Siculus, nor others among "the curious" of the classic ancients who went prying about precisely on such a quest, ever obtained a glimpse of, or even suspected, the real state of the case. They were, indeed, mightily troubled about certain other rubbish which their theories taught them must have existed once, viz., that arising from the chipping and squaring of the innumerable stones used in building the Great Pyramid; and they, viz., Strabo and his near contemporaries, have left us this invaluable note of the state of things in their day, that they walked all around and about the Great Pyramid, actually *looking for* the said heaps of chips and mason's rubbish, but could find none. The building seemed to them "as if it had descended from heaven complete at once, and without the agency of man,"—certainly, we might add, without the agency of the *modern Egyptian* man; for, go where you will now about Cairo, you see rubbish wholesale wherever there is, or has been, any building.

Each age of the world seems to have selected some special feature of the Pyramid to wonder at, and in every case the real full fact has subsequently been found to be still more wonderful than the portion that had first been recognised on the surface. Thus with Strabos's little wonder; so utterly incomprehensible was the complete removing of all the mere mason's rubbish at the Pyramid to him and other archæologists of 2000 years ago, that they invented the most startling accounts of the water of the Nile having been led *uphill* to wash away heaps supposed to be impossible to man.

Yet 2000 years more in the passage of time have revealed not only where all that rubbish was neatly stowed away by the hand of the builders, but the much larger amount also which arose from their masterly preparation of the summit of the hill for the honourable burden it was in future to bear. For both supplies may now be seen—detected by the differential effect of occasional rain through so many ages on their loose heaps, as compared with the compact natural rock—banked away against the northern cliff of the hill. Banked up too, so firmly against it, and so nicely levelled and smoothed on the top, as to have made for 2000 years at least an undistinguished extension towards the north of that flattened area of worked rock surface on which the Pyramid immediately stood.

An extension, however, that was in so far decidedly the right thing in the right place; for, so close is the Great Pyramid to the northern brink of the hill, as almost to threaten danger to its foundations on that side. A safer site might have been found further south and west on the same hill, or on the southern one, or on the so-called Arabian hill on the eastern side of the Nile,—whence, too, the builders had, even as it was, to derive all their best qualities of stone for facing the outside of the Pyramid and lining its interior passages. But they preferred that spot, whereon the Great Pyramid now stands, with its one only passage of communication with the exterior air looking right away northward from the very brink of the cliff, without any interruption, over the fertile delta land of Lower Egypt.

Now this opportunity of free and open view in that direction would not have been commanded from any of those other sites; and the Great Pyramid has wholly, if not in a manner visibly, arrogated this particular and *unique* site to itself after a most remarkable fashion. For all the other pyramids of Egypt are, by their places, kept away southward of it, or behind it, in its lee as it were, forming a ruck of lame and impotent ones; or a servile suite of attendants waiting silently on their Lord, while he advances in calm and single majesty to indulge in his own thoughts as he gazes northward from the end of the high rock, where it looks down into, upon, and over the river-formed and populous, triangular plain. One unfortunate, ill-omened, pyramid was indeed begun in advance of the imperial monument of the land, or about six miles to the N.N.W. at a place now called Aboo-Roash. But never did exception better prove a rule; for *that* pyramid has remained ever since miserably unfinished; and, whatever the reason was, all men may go to the place now,—trace by the large ground plan, and steep angle, the ambition of the design,—but on looking at the few feet of height accomplished, and the small portion of building material lying about, they may say with supreme significance, and with as much certainty as though they had been eye-witnesses of what took place 4000 years ago, "This man began to build and was not able to finish."

Thus much any one can see for himself with his own natural eyes, standing in front of the Great Pyramid and looking northward over the flat country which is spread out diverging before it, as if for its pathway to larger power. But the good fortune was reserved to Mr. Henry Mitchell, Chief Hydrographer on the United States Coast Survey, to show further, from maps and charts, that the position of the Great Pyramid is at the very centre of origin of both the mathematical figure, and even physical formation, of the whole delta-land as an angular 90° *sector* in shape; also that the Great Pyramid's central meridian line, or the plane of its one entrance-passage continued, forms the meridian line of all Lower Egypt, marking precisely the longitude of the northernmost and culminating point of its regularly curved northern coast; while the N.W. and N.E. diagonals of the building produced, similarly define the two sides of the delta, stretching though they do far beyond eye-shot from even the summit of the building; and all the fan-like system of streams which irrigate that most fertile of level lands, all, when viewed from without, point convergingly towards the Great Pyramid as their one centre of command.

Mr. Mitchell, indeed, working from the convex curve of the northern coast-line on the charts to find the centre of formation thereof, some 120 miles inland, confesses that his arcs and radii do not pretend to distinguish between the Great Pyramid and its immediate fellows on the hill of Jeezeh, though that general group they do determine. But the moment we visit the hill itself, thus centralised, there is not a particle of doubt left then as to which is the master building there, viz., the Great Pyramid,—for it alone, as just described, stands over the sector's circle's centre, and possesses the one commanding prospect, extending, without interruption, from its very foot illimitably away towards the north, and north-east, and north-west.

Even further still, too, as Mr. William Petrie has lately shown by careful calculation,—the actual, *i.e.* the adjusted, height of the hill around the base of the Great Pyramid, *also* marks for Physical Geography that precise hypsometrical level to which the whole ocean surface would rise, if every particle of presently sub-aërial land were to be distributed over the bottom of the sea; and which surface of such terraqueous globe is that to which geodesists should refer all their measures for the true and full size of the earth.

"Never," therefore most truly may Mr. Mitchell exclaim, "never was there any building founded by man on so important a physical site," or with the whole surrounding country serving in all its natural features merely as a chorographical pointer, on the grandest dimensions, to the said building's eternal place of standing on "the utmost bound of the everlasting hills," during all human history.

Nay, indeed, there is even yet *more* of hard scientific fact to distinguish the position; for, on recently summing up the areas and positions of all the land-surface of the entire globe,* it has been found that the general superficial centre thereof (*i.e.*, of the *land surface* or that which mainly constitutes the greatness of human empire and the numerical strength of nations), such centre,—when measured both in latitude and longitude, and defined also by the crossing of the longest line of land surface in latitude and the longest in longitude,—comes, in either way, close to the same identical Great Pyramid site. No other natural position, therefore—if required now to be sought for whereon to establish a central anthropological monument for all the human race through all historical ages—could be found, other or better than this remarkable one in Lower Egypt on which the Great Pyramid is already, and has been so long, established with such care and precision in all particulars.

4. *Shape of the Great Pyramid.*

The levelling of the rocky hill top and the production thereby of a white and smooth flat surface of a half chalky but compact limestone, was of course a famous preparation for laying off any mathematical figure with exactitude, even when of the grandest dimensions; and it was done.

At the north-east, south-east, and north-west angles of the huge square base of this pyramid, the corners are marked by flat-floored, shallow, rectangular sockets to hold the colossal corner foundation-stones of the whole building. But at the south-west angle, where, too, the socket, though dug for through the modern superincumbent rubbish in vain by Mr. Inglis (Mr. Aiton's assistant) and his Arabs, including amongst them two "sheiks of the pyramid," was only at length discovered when I lent them the aid of the late John Taylor's theory, by measuring his computed angle from the less dilapidated summit of the building, and showing them thereby on the ground below, where the outside corner of ancient times *ought* to be. There, then, when that long-lost socket was thus re-discovered in April, 1865, after a repose of 900 years, was its floor, cut methodically into the fair white rock, and levelled so accurately as to be nearly without error for placing a modern altazimuth instrument upon; and there, too, were the chiselled-out, elevated borders of the socket on three sides, but on the fourth, only a line, an indented line about 0·08 inch broad and 0·02 deep, ruled on the flatted rock surface: but a line so straight, so true, so steady, and so firm, that it proved itself at once the work of a master hand.

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* See "On an Equal Surface Projection for Maps of the World," by C. Piazzi Smyth. Published by Edmonston and Douglas: Edinburgh, 1870.

We could, moreover, hardly fail to recognise whose hand it was, so precisely similar appeared the line in all its finer graphiological features to two other lines which I had already discovered on the walls, far down the dark vault of the entrance passage of the Great Pyramid; lines introduced there, too, in a manner that showed they must have been the veritable work of the original builders, and were well adapted to call attention to a singular change required at that point, for apparently symbological purposes, in the angular direction of the joint lines on either side.

Thus closely, then, have we of the present day all four corners of the ancient square base still accurately defined; though, alas! for modern science and scientists too, not yet measured by them with sufficient exactness. For, although the greatest linear error, in terms of one of the sides as unity, has been given by the only series of socket measures yet published in full,* as being not more than 0·001, still the greater part of that quantity may be shrewdly assigned as the "probable error" of the observations themselves. And similarly with the only observation yet published of the angle at one corner included between two adjacent sides, and which came out within 0·0002 of the required right angle.

The base, therefore, of the Great Pyramid is, to fully as close as modern measures have yet been carried, a sensibly perfect square; and it only remains to ascertain, for the due definition of "shape," what is the angle of rise of the triangular sides, or flanks, of the building, from the four edges of the said base.

This angle, the builders could make anything that they pleased under 90°, and their structure would have still been a true mathematical pyramid. Or, again, they could also, if they pleased, though in that case to the destruction at once of the pure mathematical idea, have begun by raising up from the base an ornamental rectangular pedestal, with vertical sides, fluted mouldings, and other trickery of the art architecture of subsequent days and human schools; as in fact, too, some latter-day writers, thinking no grand architecture possible without ornament for ornamentation's own sake, have evolved out of their own internal consciousness and applied to the Great Pyramid. But Colonel Howard Vyse's happy discovery of two of the lowest row of that pyramid's original outside casing-stones still *in situ* near the middle of the northern side of the base, and beginning their angular slope at once from the broad flat "pavement" there, has demonstrated the Great Pyramid's general form to be decidedly such as to illustrate an everlasting geometric truth, rather than merely to adorn an architectural fancy of the passing hour.

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* See "Life and Work at the Great Pyramid," by C. Piazzi Smyth. 1867, vols. ii. and iii.: Edmonston and Douglas, Edinburgh.

The Colonel's discovery gave also an excellent opportunity, so marvellous were both the original exactness of the workmanship and the perfect preservation of those great bevelled blocks of stone, to determine their precise angle of shape; and he made it by two different methods,—not, however, without a certain rather puzzling small error therein,—to be somewhere between 51° 51′ and 51° 52′.

This was, neverthless, as compared with all his predecessors, most respectable work; and yet when its mean result was employed soon after, as giving the Great Pyramid's whole angle,—certain London critics exclaimed, and not without some wit, that that was rather too exaggerated a case of deducing the *ex pede Herculem*, for them to swallow. But they need not have disturbed themselves: for, as I have shown in Part I., p. 33, my own long subsequent observations of all four of the grand arris lines, comprising the entire Pyramid within them, gave for the mean sides the equivalent of 51° 51′, and some seconds.

Or, if other proof of more multitudinous character be required for the universal public, they have only to dig in the heaps of broken material encumbering now every side of the Pyramid, to find innumerable fragments of the casing stones which once covered the whole of the tall triangular flanks: and among these fragments they will often find precious examples with remnants of the outside bevelled slope in connection with either the upper or lower horizontal worked surfaces. I myself found no less than nineteen such pieces, or two from the south, five from the west, five from the east, and seven from the north side, and they gave the mean angle as between 51 °49′ and 51° 58′. While a more remarkable and unexpected testimony still was derived from measuring the azimuthal angles of an otherwise incomprehensible system of far separated and huge trenches cut anciently into the rock of the hill before the eastern face of the Pyramid, so as to have a joining of all their axes *when produced* in one central point, and there giving up, to the alt-azimuth instrument which interrogated them in 1865 A.D., the angle 51° 51′ 35″ for the foot, and 76° 18′ 37″, or within a minute or two thereof, for the summit angle of the Great Pyramid.

5. *Size of the Great Pyramid.*

What so easy, at first sight, as to determine the lengths of the four sides of a square base, marked off neatly by corner sockets, or even by ruled lines on a flat and almost polished surface of white rock; and yet it has not been done by modern men in this case even up to the present time?

But let the reader please to remember, that though two of the sockets were indeed discovered so long ago as in the year 1799, by the French *savants* under Bonaparte, the other two were only brought to light by Messrs. Aiton and Inglis, and partly by myself, in 1865. And, even then, though all the four sockets themselves were, at that time, for some days, bared at once, and brushed quite clear and clean of all dust; yet, when a would-be measurer stood at, or upon, or within one of them, instead of finding the line from that socket to the next one smooth, level, and proper for laying his measuring rods upon,—behold a long hill of rough, ruined, stone rubbish near sixty feet high, occupying the middle part of the line, and a similar one on each of the other three lines.

Who, especially if limited in time and means, could measure accurately over such obstacles as those? Certainly the men who have tried it have not covered themselves with glory. For Mr. Inglis's measure of the mean of the four sides in 1865 came out 9110 British inches; and that of the ordnance officers in 1869 came out 9130 British inches; while the much more careful and elaborate measure of the French *savants* of 1799,—on only one side; indeed, but of the assumed square, and where they had two terminal sockets to measure between,—was 9163 inches; and the repetition of the same by Colonel Howard Vyse and Mr. Perring in 1837 gave 9168 inches.

Now, all these numbers cannot be correct, nor even each of them for its own date; for base lines, so marked in solid rock, are not elastic and variable through enormous limits. What, then, is the one and only real, true, and definite length of the mean of the four sides, as it is in nature and fact, to an accuracy let us say of a tenth of an inch?

Unfortunately, the whole modern world can give no satisfactory answer to this simple question. Our own country, even by itself, has spent millions of money on their Home survey, the Indian survey, and surveying in the Colonies. It has spent largely, too, on surveying the archæology of various lands, and is spending most liberally at this very moment on that of India; but on the measurement of the earliest archæological monument of the whole world, and that which lends itself at once, by the whole nature of its form and construction, most absolutely and completely to become the subject of exact mensuration, viz., the Great Pyramid, our rulers have not thought fit to spend a farthing; unless, indeed, some of them are responsible for that mere spurt of an effort made in 1869 by the officers who were returning from the survey of Sinai, and whose probable error in one of their base-side measurements was apparently something more than 300 times as large as what was incurred on a base line measured in India, about the same time, by the officers of the trigonometrical survey there.

But what are we, who desire to theorise on the Great Pyramid, to do at the present moment with these conflicting modern numbers before us, for the true length of the Pyramid's ancient base-side?

The simple mean of the whole, or 9143 inches, might be a fair approximation; or, again, weighting each result according to the care and means employed in obtaining it, perhaps 9160 inches might be closer to the truth. But on neither of these numbers, nor any neighbouring ones to them obtained by similar judgment processes, do I venture to decide absolutely. I only insist that we are bound to hold that the true quantity must be somewhere between—perhaps not far from midway between—the extreme limits of the widest observers; such conclusion being, moreover, also borne out by a comparison of all the best measured heights of the Pyramid, as determined by various methods, and used, in combination with the angle of slope, for deducing the length of the base side.

And then, in conclusion of this part of the subject, to show that we have, after all, some valid grounds for confirming the popular feeling of all the world throughout all ages, for the so-called "Great Pyramid" being really such,—I subjoin a list of all the Pyramids of Egypt, similarly measured—and it will be found that there is not one which contests the Great Pyramid's superior size, within many times even the most extravagant estimate yet made of the probable error of the measures.

THE PYRAMIDS OF EGYPT;

ALL STANDING IN THE LIBYAN DESERT, ON THE WESTERN SIDE, AND ABOVE THE LEVEL, OF THE NILE VALLEY; THE MEASURES BEING APPROXIMATE ONLY, AND CHIEFLY BY COLONEL HOWARD VYSE AND MR. PERRING.

Modern name of each Pyramid.	Latitude. North.		Angle of rise of the faces to horizon.		Length of side of base, always four-sided.		Central axis, or vertical height.	
					Present.	Ancient.	Present.	Ancient.
	Deg.	Min.	Deg.	Min.	Brit. Inches.	Brit. Inches.	Brit. In.	Brit. In.
Great Pyramid of Jeezeh	29	59	51	51	8950	9142 9168	5410	5819 5833
Second Pyramid of Jeezeh	29	59	52	20	8290	8493	5370	5451
Third Pyramid of Jeezeh	29	58	51	0	4200	4254	2436	2616
Fourth and minor Pyramid of Jeezeh	29	58	in steps		1230	2200	834	1440
Fifth ditto ditto	29	58	52	15	1656	1749	1000	1119
Sixth ditto ditto	29	58	in steps		1230	2200	834	1440
Seventh ditto ditto	29	59	52	10	1500	2070	540	1332
Eighth ditto ditto	29	59	52	10	1500	2070	660	1332
Ninth ditto ditto	29	59	52	10	1440	1920	960	1221
Pyramid of Aboo-Roash	30	4	no casing		3840	*x*	480	*x*
Pyramid of Zowyet el Arrian	29	57	ruins only		3600	*x*	730	*x*
Pyramid of Reegah	29	56	75 50	20 0	1200	1480	500	1150
Northern Pyramid of Abooseir	29	54	51	43	2600	3084	1400	1953
Middle Pyramid of Abooseir	29	54	51	(?)	2560	3288	1284	2056
Great Pyramid of Abooseir	29	54	52	(?)	3900	4317	1970	2734
Small Pyramid of Abooseir	29	54	50	(?)	650	905	216	564
Pyramid 1 of Saccara	29	53	rubbish only		2500	*x*	700	*x*
Pyramid 2 of Saccara	29	53	52	(?)	2150	2775	1300	1758
Pyramid 3, or Great Pyramid of Saccara	29	53	73 in steps	30	3700 N. to S. 4200 E. to W.	4214 N. to S. 4727 E. to W.	2200	2405
Pyramid 4, at Saccara	29	53	ruined		2640	*x*	740	*x*
Pyramid 5, at Saccara	29	53	ruined		3000	*x*	480	*x*
Pyramid 6, at Saccara	29	53	ruined		3240	*x*	960	*x*
Pyramid 7, at Saccara	29	53	ruined		1680	*x*	330	*x*
Pyramid 8, at Saccara	29	53	ruined		2880	*x*	1044	*x*
Pyramid 9, at Saccara	29	53	ruined		2940	*x*	900	*x*
Pyramid base of Mustabet el Faraoon	29	53	in steps		3500 2300	3708 N. to S. 2604 E. to W.	650	720
Northern *Brick* Pyramid of Dashoor	29	49	51	20	4500	4200	980	2586
Northern Stone Pyramid of Dashoor	29	49	43	36	8400	8633	3918	4111
Southern Stone Pyramid of Dashoor	29	48	54 42	15 59	7400	7400	3834	4029
Small Pyramid of Dashoor	29	48	50	12	1700	2172	816	1281
Southern Brick Pyramid of Dashoor	29	48	57	20	4800	4110	1872	3208
Northern Pyramid of Lisht	29	38	ruinous		4320	*x*	1080	*x*
Southern Pyramid of Lisht	29	37	ruinous		5400	*x*	822	*x*
The False Pyramid, or Pyramid of Meydoom	29	27	74	10	2388	*x*	1494	*x*
Pyramid of Illahoon	29	17	ruinous		4320	*x*	1580	*x*
Pyramid of Howara	29	18	ruinous		3600	*x*	1270	*x*
Pyramid 1 of Biahmoo	29	26	63 50	30 0	360	1440	360	*x*
Pyramid 2 of Biahmoo	29	26	63 50	30 0	360	1440	360	*x*

6. *Internal Arrangements.*

Had the above tabular statement been extended so as to contain descriptions also of the interior passages and chambers of the Pyramids, more striking diversity still would have been found between the great monument and its companions. For though all of them have a descending entrance passage with a chamber at the further end, *i.e.*, the simple tombic arrangement known to all men, none but the former has an *ascending* passage and *other* chambers high up in the sub-aërial masonry of the building.

That ascending passage, with its consequences, is indeed the chief constructive secret, as well as peculiarity, of the Great Pyramid; and a secret well kept too, for with the exception of its place having been broken into once, apparently by some very ancient fanatics of the native race, the mystery remained unknown to all other men, from the building's primeval birth down to the year 800 A.D., when an accident, often described in books since then, disclosed the beginning of the upward ascent to the Khaliph Al Mamoon, who was quarrying at the moment close by.

Yet was the arrangement not *intended* to escape posterity for ever; seeing that so soon as I began to measure the important, and otherwise regular, joint lines in the floor of the descending passage both in mathematical manner and in a respectful frame of mind for the original designer (a simple combination, but which seems to have escaped previous travellers), than I stumbled on, nay, started at, rather than merely discovered, two lines that were altogether anomalous in their angle, as well as peculiar in the extra hardness of their stone material; and behold! they pointed to the very place where, behind an apparently ordinary part of the ceiling, and totally unsuspected by later Egyptians, Ptolemæan Greeks, and Imperial Romans (who *did* pass up and down the more ordinary descending passage) the hidden *ascending* tube begins to take its mysterious rise. More yet, too, of such hints had been left behind; for only a little way beyond the system of horizontal-angle trenches, already alluded to, in front of the Eastern Side of the Great Pyramid, there is a full sized model, in everything but length, of the said Pyramid's unique system of both descending and ascending passages, cut into the rock in a vertical plane; and with even a beginning of the greater breadth and peculiar ramps of its grand gallery.

But if the Great Pyramid has these *additional* hollow spaces over and above what other Pyramids possess, must it not thereby be just so much further off from the ordinary mathematical definition of a Pyramid, viz., a *solid* of such and such a shape?

True is the objection in principle, though in practice not felt to any sensible extent; and because, in the first place, all the hollow portions of the Great Pyramid put together barely amount to 1-2000th of its whole bulk; and in the second place, so far as specific gravity is concerned, the effect is in part made up for by some of these same interiors being lined, and others even filled, with *granite*, a far heavier material than the very light nummulitic limestone of the general mass.

And here let it be noted for a moment, *how* the Great Pyramid makes use of that remarkable material, granite; viz., in the very uniform temperature of its *interior*, and there only; where too it has lasted and preserved its anciently polished surface admirably. When my friend St. John Day, C.E., in a lecture to the Philosophical Society of Glasgow, was recently describing the Mokattam limestone which the Pyramid architect had chosen for its *external* covering, where the changes of temperature between night and day, under the radiations of sun and sky, are excessive—a practical constructor rose with exceeding positivism to say:—"Now, I think that the architect was wrong there, and I am sure that I could have advised him better; for I have come to believe that all the strength of the earth consists in granite Divinely put into it; and when the town council the other day took up a pavement that I had laid for them ten years before with granite out of the quarries of his grace the Duke of Argyle, it was found perfectly sound and good after all that wear and tear."

Mr. Day's answer, by merely relating facts from the Pyramid, was as ready as if prepared beforehand: viz., that the architect of the subsequent third Pyramid (decidedly, too, an idolatrous and anti-great Pyramid man) *had* used granite for the external casing of a large part of his Pyramid; and with the result now, or after the more certain and crucial test of 3500 years, that the exposed granite surface is so disintegrated or exfoliated, especially at the corners and edges, that the once smooth-planed bevelled casing blocks are now in shape like so many puddings, or even embryo boulders; and fail, in fragments, to tell the original angle they were cut to, within many whole degrees. While the peculiar limestone of the similarly *external* coating but of the still *more* ancient Great Pyramid has had such a special power of resisting atmospheric effects, that you may occasionally, among the later ruins of it caused by man, pick up specimens where the meeting of two of the worked surfaces is preserved sharp up to their mutual corner line, without a deficit of more than one or two hundredths of an inch broad.

Whereupon another C.E. expressed soon afterwards the effect produced on *his* mind, by the ultimately right rock for the place and purpose, though at the moment, from its softness, so utterly unlikely to ordinary apprehension (and there were *then* no previous stone buildings in the land to refer to for experience), having been so unerringly chosen by the designer of the Great Pyramid. But we can hardly delay now for mere mental testimony, seeing that details of numerical mensuration are our true subject in this place, and are quite as much as we can pretend to overtake.

Nay, indeed, even in that department also, the editor's limits of space begin to warn me to contract the fuller account that I had once hoped to give; and perhaps I may be allowed all the more readily by the reader thus to do, seeing that in so far as the *interior* of the Pyramid is concerned, the battle of the different mensuration testimonies extant, was in reality fought out by me in 1865, at the place itself; and by the only unexceptionable method, viz., that of measuring the parts disputed again and again; by taking fifty measures where my predecessors had taken only two or three; by working sometimes six hours a day in the solitary interior of the dark monument with only my candles, measuring rods, and note-books about me; and by continuing such studies day after day through several months, instead of paying the place a single visit of only a few minutes' duration, and then, either oppressed by Arabs, or thronged by "troops of festive friends."

Wherever the subject of measurement on those occasions exhibited to me original worked surfaces of stone in tolerable preservation, I attempted to make all linear measurements to tenths of inches; but if the ancient surfaces were superfine, then to hundredths of inches; while the angles of the descending and ascending passages, together with others for testing the reputed horizontality or verticality of floors and walls, were examined by three different classes of instruments, of which, if one was characterised chiefly by portability, the other two were splendid examples of their kind for accuracy: viz., first, a clinometer, presented to me for the occasion by A. Coventry, Esq., of Edinburgh, with its base, 120 inches long, and a central complete circle for the angles, reading by three pairs of opposite verniers to ten seconds each; and second, an altitude-azimuth circle, donated years ago to the Royal Observatory, Edinburgh, by the famous Professor Playfair, and reading off both its circles by micrometer microscopes *to tenths of seconds*. In order to convey these large instruments into the Pyramid's interior, I had of course, then, to hire several Arab assistants; but the days for their use came few and far between; partly, too, because I had, as my experience advanced, to prepare with my own hands wooden stands and other helps to observation adapted in shape and size to the strange corners, slopes, projections, &c., where the instruments required to be placed.

But all these painful and laborious steps having been duly gone through, and the separate measures, whether accordant or discordant, printed in full elsewhere,* the reader may not care for more now than merely to gather some of the cream of the results, and hear what they seem to tell.

PART III.

OF WHY THESE THINGS WERE THUS MADE OF OLD.

1. *Wherefore the Position of the Great Pyramid.*

Perhaps the title of this third part is a shade too presumptuous; at least, for the safe and cautious mode on which alone I propose to proceed.

For example, touching our very first topic, or the *position* of the Great Pyramid, have we not already shown that that site is in the very centre of all the land surface, or man-producing and man-supporting regions of the whole globe; and is not that quite a sufficient reason, for *there*, rather than anywhere else, having been placed of old,—if sufficient wisdom presided over its birth,—that one vast monument which is full of instruction to man, and not only saw the beginning of human history, but bids fair to be a witness also of its termination.

True that such central position, though in so far a fact, *may* be only an accidental coincidence; especially as it requires a knowledge of Australia and America to make it even approximately true. But then there is a sort of triplicity in the fact itself which gives a certain *small* probability in favour of intention, from knowledge derived somehow or from somewhere, to begin with, in rolling up our ball of pyramid scientific ideas, purposes, and intentions.

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*Vol. ii. of "Life and Work," by C. Piazzi Smyth. Edmonston and Douglas, Edinburgh.

For, if geography thus assigns the general central position of the land-surface of the world roughly to Lower Egypt, chorography assigns a particular physical centre of figure and formation to a certain part of Lower Egypt, and locates that more precise point upon the hill of Jeezeh; while minute topography there shows the Great Pyramid arrogating peculiarly to itself the one only exact standpoint on that hill which distinctly commands the beginning or central origin of the peculiar physical domain just mentioned.

2. *What Decided the Shape of the Great Pyramid.*

Who does not give precedence to German minds, and especially to their greatest leaders in philosophy, in every question of deepest mystery. Can we then pass over what the illustrious Schelling has written on the identical subject of this section. Certainly not, for it is exemplary in every way.

"The Great Pyramid of Cheops," says Schelling,* was the symbol of the higher Monotheism, in which way we can account for its being constructed on a quaternary principle, viz., on the three powers of Typhon, Osiris, and Horus (below), and on the fore-mentioned one-only-Godhead (above). (The three powers form the basis, the one God above them the apex.) For the quaternary is also the ruling principle in this theistic system, viewed as intelligible (although as yet we have only developed it into the three-fold), as already eight gods have come into view out of Herodotus, which, when one understands the half to be female, shows that the fundamental number is the four-fold. The pyramid is the first body, the first solid, and since, in the old philosophy of arithmetic, the point stood for unity, as the line appeared to be produced out of the number two, and the plane superficies out of the number three, so the grand meaning of the number four was seen in this, that it would show itself as the first solid-like number, since the pyramid, the first of the five regular bodies is produced in it with the four given points."

Now there is a great deal in this essay most admirable as to some general principles of philosophising, though in the particular instance before us, failing miserably in practice. Not only, too, because we have no proof that the myths of Typhon, Osiris, and Horus were invented until several centuries after the Great Pyramid, but because we do know positively, even from our own careful, clear, distinct, and broad day-light measures, that the Great Pyramid's base, which these hands have handled and these eyes have seen, has not three, but four, sides; and that the whole Pyramid has therefore of necessity not four, but five, points or corners, and five bounding sides or surfaces, the base being mathematically and crystallographically included as one.

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* "Philosophie der Mythologie," pp. 405, 406. From J. T. Goodsér's "Homilies," p. 260.

Hence springs up not a quaternary, but a quinary, arithmetic; and a quinary, too, so combining two sets of five into one whole, as to lead on just as naturally to a complete decimal system, as the two so-called five-fingered hands belonging to one body of a man are usually held to *have* led to the world's present decimal arithmetic. The parallel, too, is all the closer, because, if the human hands contain really not five, but four tapering fingers and one broad thumb, does not the Pyramid exhibit four taper triangles and one broad square. Or, again, to take it in a higher manner by its points or corners, are there not four lower, earthly, ordinary foundation corner-stones, and one upper, topmost, skyward, unique head-corner-stone,the very "head of the corner," and in which, as we read in a higher than any German authority, "all the building fitly framed together, groweth unto an holy temple in the Lord."

Poor Schelling was, therefore, in a manner not so very far out after all, at least as to having a vision of certain principles; but he should assuredly have tabled the number five in place of four, and ought to have been thinking about the one true God of Scripture, and not of the unlawful theotechnic inventions of later idolatrous Egyptian minds. Had he only so acted, he could at once have counted St. Paul, St. Peter, David, and Isaiah (themselves lower foundation corner-stones) on his side,* and would have enjoyed a priority over the late John Taylor† in giving the modern world their first full interpretation and only perfectly satisfactory mechanical explanation of the figurative expression both of the Old and the New Testament "head-corner-stone;" for it can be exactly paralleled nowhere else than in the capping-stone of a pyramid similar to the Great one.

There yet remains, however, the question as to the *angular* slope of the Great Pyramid's sides, for that forms equally a part in the decision of *shape*.

* Psalm cxviii., 22; Isaiah xxviii., 16; Zechariah iv., 7; Job xxxviii., 6; Ephesians ii., 20, and iv., 15, 16; 1 Peter ii., 4, 5, 6, 7; Luke xx., 17, 18; Matt. xxi., 42, 44.

† Long the publisher to the London University; died in 1864. The book here referred to is by name "The Great Pyramid; Why was it Built, and Who Built it." Longmans and Co., London.

That angle, too, might have been varied at the pleasure of the architect (with no precedent all the world over to control his proceedings), from the almost flat appearance of a mere "buckled" plate, or of King Alyattes' low and broad tumulus mound in Asia minor, up to a needle-shaped spire to compete in subsequent days either with Pagan Egyptian obelisks or modern Christian church spires, for delicate slimness of figure. But the said architect chose to give his building the special form due to an angle of 51° 51′ and some seconds; and kept to that one angle with singular care and success.

What virtue, then, if any, was there in that particular angle as introduced in that place?

To the same venerable John Taylor, in his 80th year, we owe also the happy suggestion, that it was because that angle, in such a quinary pyramid, is demonstrative of the true "squaring of the circle," known to mathematicians as "the value of π," a subject which is at the base of all practical calculations throughout the whole of the exact sciences; and though the quantity be styled an "incommensurable" (that is, an unlimited eternal fraction), yet has its true value, approximated to only by the Greeks, been obtained at last correctly by the learned of modern Europe to at least thrice as many decimal places as are ever required in practice. Their result, too, stands perfectly firm, though it is still ever and anon being radically disputed through its whole extent by those strange enthusiasts who never die out amongst men, but are continually reappearing in almost every month's serial literature that so quickly passes on its inevitable road to oblivion.

According to John Taylor's supposition, 51° 51′ 14·3″ is the precise *computed* angle which such a π pyramid ought to possess, and is just about as close to a mean of the best *modern measures* yet made of the Great Pyramid, as is possible for *their* residual errors. Closer, in face of the growing dilapidation, no future measures can well be expected to come. Hence, any further proof that π and not some other idea, differing in its angle by a few seconds only from the π quantity, was intended, must be sought elsewhere than in practical re-measurements of the simple angle. Nor have we very far to seek.

On the π principle, the continued length of the four sides of the base is equal to the straightened-out length of the circumference of a circle whose *radius* is the vertical height of the pyramid. If such a circle be drawn concentrically on a plan of the Great Pyramid, its circumference will be found to lie outside the base when near the middle of the sides, and inside at the corners. By just such a quantity, moreover—if the pyramid be taken in progress of construction, or without its final sheet of casing stones—that the projection of the circle at the middle of the side has been shown by W. Petrie to be coincident with the common axis of two of that remarkable system of azimuth trenches just mentioned; while rectangular offsets from the places near the corners, where the circle's circumference enters the square base, define the very precisely cut and broad rectangular ends of the two self-same trenches.

After this, it is needless to add that Herodotus's puzzling statement of the height of the pyramid being equal to the length of the side of the base, when interpreted by John Taylor as referring to *square* measure, and meaning that the area of one of the sides was equal to the square of the height, does really give as its angle 51° 49′, or closer than many of the modern measures; and that a very lately started idea, or that a rise of nine for every ten of distance in the diagonal of a pyramid's base, would give a quantity differing only a few seconds from 51° 51′ 14″—all this I say is needless, because these ideas with merely closely similar but not identical angles, and many other ideas which are vastly simpler—but vastly further off, too, from the angular truth as measured—have no such additional testimony of intention from the builders themselves, as the π theory has. *Inside* the pyramid, too, we may truly say, as well as *outside*, for a corollary of the same theory assigns the angle for both the descending and ascending passages; and when the longest length and most carefully constructed part of any of them, viz., the Grand Gallery, came to be measured by myself,—with instruments, both mechanical and astronomical, the like of which for power of accuracy had never been taken inside the Pyramid before,—the mean result, though in the hands of some previous explorers differing several degrees, was 26° 17′ 32″, the computed angle being 26° 18′ 10″. While the closely approximate π value itself is found again, permeating both the peculiar arrangements of the antechamber* and the proportions of the *coffer*,† that ultimate treasure of the final and innermost "King's Chamber" of the Great Pyramid.

Let it be further noted, too, by the assistance of the tabular view of pyramids already given, that not one of the others has this angle or anything near it; no angle either for which any superior mental idea has ever been proposed. The builders of those other and subsequent pyramids copied the Great Pyramid in all that *they* saw or *they* thought important; but plainly the scientific secrets, as reasons of its construction, were not known to the ordinary idolatrous Egyptians of even the most ancient days. Of course they could not fail, if copying at all, to reproduce the *square* base and *four* triangular flanks, whence they may lay claim to quinary arrangements and decimal arithmetic, which, in fact, the old Egyptians had. But the *angle* of the Great Pyramid! No, they never hit that! Nor even imagined that there was anything important therein! For over and above the utterly diverse *built* angles of so many of the subsequent pyramids, there may be seen still, even in the best cut hieroglyphics and Royal cartouches of pyramid-building kings of ancient Memphis, little figures standing for pyramids and most ridiculously out in their angles; being, generally, tall and slim or anywhere between 60° and 80°, and this by artists who could touch off the precise outline of any of a hundred varieties of animals to most speaking likeness and almost microscopic exactitude.

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* As shown by Captain Tracey, R.A., in a paper about to be read before the Royal Society, Edinburgh.

† St. John Vincent Day, the discoverer. See his published "Papers on the Pyramid;" also his folio book "Plates and Notes on Pyramids."

So far as *shape* is concerned, then, all these other pyramids came no nearer to the Great Pyramid than do monkeys to man made in the image of God. Monkeys have at least as good, if not better, five fingered hands than man; but soul and immortal spirit and capacity for religious impressions—where are they?

Besides, however, its correct solution of the universal π problem, and its claim, considering also its primeval date, to school respect on that account—the Great Pyramid is further, and even thereby, an instrument for holding up to man, through every age, a certain thing in creation which he, with all his powers, cannot evenly decimalise, strive he though never so wildly. And yet, viewed submissively according to nature's laws,—the very self-same principle gives the building a claim to distinguish and typify some other numbers, especially 3 and 7,* besides the 5's and 10's of its radical formation, which lead to some singularly instructive consequences.

* See postscript by W. Petrie to vol. iii. of my "Life and Work at the Great Pyramid."

3. *Why of that Particular Size.*

If we next inquire why the Great Pyramid was made so truly great as modern measure proves it to be, the ready answer has too often risen up in polite society, from the time of the elder Pliny to our own,—" Because the man who built it was a brute; he appreciated nothing but big, burly, bullying size, and he tyrannised over his people and drained his country of its resources, merely to make a vulgarly grosser tomb for himself than his neighbours had."

A very pretty theory, too, is this, as it stands; but then it does not explain in any degree either the remarkable abnegation of praise, vain-glorying, and self which was alluded to in Part 1, page 30, or the already proved fact of the recondite and most meaning *angle* that was adopted and rules the whole building from bottom to top, and from side to side. Now so important and all-pervading a feature of shape as that, not only cannot be overlooked, but we ought to take it along with us when seeking an explanation of size also. And how shall we consider the size chiefly exemplified? By one of the four base sides? Or by one of the four longer diagonal arris lines? Or by one of the four slanting heights from the middle of a base side up to the apex? By none of these mutually confusing and competing measures; but, inasmuch as the shape-theory has already attached such importance to the *one* and only line representing vertical height, and made that to be radius,—why we will take it for size also as a radius; but a radius of what?

Next to land surface for man to dwell on, what is there so important to his physical well-being on earth as sunshine. " The Sun; Ruler, Light, Fire, and Life of the Planetary System," is the well thought out title of Mr. Proctor's recent solar book, and applies most eminently to the earth and man's corporeal existence thereon. So, too, had also considered, before that book was written, my friend W. Petrie, and had convinced himself from other sources as well, that in so anthropologically directed a monument as the Great Pyramid, there *should* be an expression of the sun's mean distance from the earth as a radius, and that the numbers 10 and 9, or rather 10 and 3 × 3, as he preferred to write it, should play the chief part in the proceeding. Wherefore he took the vertical height of the pyramid as a radius, and multiplying that by 10 raised to the power of 3 × 3 (*i.e.*, 1 with nine o's after it), got 92,080,000 miles for the result. But he regarded that, at the time, as a failure; for all his astronomic books told him that the true distance was 95,293,056 miles, and he had in consequence almost thrown the whole thing overboard, when he heard accidentally one day that within the last ten years, astronomers have entirely quitted their former trust in 95 millions and so many odd miles (though they publicly crowned with exceeding honour the German astronomer and computer who brought it out for them), and are now close upon 92 millions instead, but with no very great certainty either. In fact, at this moment the astronomical world has split into two opposite camps, one of which, championed chiefly by foreign astronomers, makes the true solar distance very sensibly greater than the other, though both of them much less than the old 95 millions. Each party is perfectly certain that they are right and the others wrong, and in their eagerness to demolish their opponents, overlook the remarkable fact that the pyramid sun-distance comes *between them*; for even with all its present needless amount of probable error as depending on the bad measures for the pyramid's size taken by modern science, both limits of this anciently defined quantity come into the vacant space between the presently opposing parties in practical and physical astronomy. They fight; but until one of them has entirely annihilated the other, the friends of the pyramid may look on perfectly unmoved.

If, then, the vertical height of the Great Pyramid was really regulated so as to be a certain round fraction of the sun's mean distance from the earth, we need not expect any other cosmical quantities to be expressed by the size of other features such as the slant height, arris line, &c., &c.; for they can only be necessary mathematical accompaniments of the settled vertical height in a π pyramid. But if we introduce a new idea, or unit of measure, something may come of it, especially if we have pyramid authority for such additional unit.

Now, in the northern hemisphere of the earth, the semi-axis of rotation stands up in the midst of the circle of the equator, something after the fashion of the pyramid's vertical height to its square base with the circular proportion. Let us take the length, then, of that semi-axis as determined by modern science and divide it by 10 raised to the 7th power,—that number being chosen partly because seven is one of the extra numbers peculiarly given to the Great Pyramid by means of its π proportion, and partly, *and* still more pointedly, because the length so obtained, or 25·025 British inches is, in the first place, signally within the limits which Sir Isaac Newton settled a century and a half ago as being the length of the sacred cubit placed before the children of Israel by Moses as the cubit "of the Lord their God;" but totally without the limits of the cubits of profane Egypt, Babylon, Nineveh, and all known idolatrous countries of the ancient world; and, in the second place, because such a length-standard is very remarkably introduced into the chamber called the Queen's Chamber of the Great Pyramid, by a massive and most unique, as well as striking, arrangement of a portion of the architecture there, which can have been brought about by no others than the original builders.

Taking, then, that 25·025 inch rod, and applying it to the base side of the Great Pyramid, it shows no less appropriate a quantity than the number of days in the year; or another most solar, most anthropological datum in astronomical science; seeing that it is the number of times the earth revolves on its axis while it circulates round the sun, with a radius vector already typified in the pyramid. At present, indeed, the roughness of modern measures of the pyramid prevents us saying with certainty whether siderial days or solar days of the earth are indicated; but both come so decidedly within the limits of the best modern measures, such as they are, that it would be hardly fair to hide what seems so close to proof in the ancient building, merely because modern measurement of it is disgracefully far behind.

Again, let us take a new unit still, *i.e.*, divide the 25·025 rod by 5, and by 5 again. A most eminently pyramid division abstractly, and practically it is justified by the pyramid itself; (1) because the chamber containing the larger unit stands on the 25th course of masonry constituting the whole pyramid; and (2) because a division of 5 by 5 again, of that very length, is shown in the so-called ante-chamber to the King's Chamber. Each of these chambers devotes one or more of their walls to memorialising a division into 5; and it is in one of them that we see first, the fifth part of 25·025 inches, and then the fifth of that again, in the outside breadth, and then the thickness of the so-called boss on the granite leaf.

Now a little mark only five inches long and one inch thick, would be a curiously difficult thing for total strangers to find in a general way throughout the Great Pyramid; it would be the prototype, indeed, of the confounding needle in the farmer's bottle of hay. But there is no such difficulty at the pyramid with this little boss; for it is taken away from the Queen's Chamber, whose walls, being only in limestone, would be too soft to retain so small a fiducial mark, and is introduced into the ante-chamber at the commencement of the *granite* constructions of the interior; and there, too, it is found projecting from the polished surface of a remarkable granite block, which is not only theoretically important from being on the junction point of several of the most signal construction lines of the pyramid, but practically is singularly prominent there, because it stands right across the one and only passage way to the further interior; and in a manner which nothing yet known fully explains, though every traveller or explorer is painfully aware of the fact, that no man can enter the ultimate and so-called King's Chamber without bowing his head low to pass under this said stone.

Armed, then, with this new unit, or the twenty-fifth part of the sacred cubit of Israel and the Great Pyramid, we measure the diagonals of the base of the whole structure, and find them amount to 25870, more or less; *i.e.*, at the rate of such unit to a year—the number of years, as well as it is known at present, in the grand celestial cycle called the "Precession of the Equinoxes."

Yet what has the Great Pyramid to do with the Precession of the Equinoxes, our readers may fairly ask; and we are enabled to answer them instantly, "A very great deal—both constructively, astronomically, and anthropologically; or as follows."

4. *Of the Chronology of the Great Pyramid.*

Not unfrequently may be traced two ideas dominating so nearly the same parts of the pyramid, that at first sight anyone might well pause before accepting either one or the other, and certainly not both of them, as having been really *intended*. Yet a closer examination usually shows, that while both ideas belong appropriately to successive steps in the same line of thought, the practical influences of each on the building remain totally distinct in their mechanical realisation.

Thus the pure geometry of the pyramid's figure, if proved at all, remains just as completely proved, after there has also been subsequently shown to have been a practical astronomy at work there likewise, settling the orientation of the self-same figure; *i.e.*, directing the sides of its base upon the cardinal points of the earth and sky.

That such geographical astronomy was no accident is abundantly shown from the excellence of the resulting emplacement; the mean error coming out, when measured by myself with the Playfair alt-azimuth instrument—(1) on the socket line of the east side of base; (2) on the same line of the north side; and (3) on the axis of the entrance passage, at something less than five minutes of angle: and these limits will probably be smaller when the chief line on which the architect relied, viz., the axis of the "Grand Gallery," can be measured in azimuth as I did measure it in altitude.

Again, my suggested geometrical diagram depending on the π proportion, and producing the passage-angle already alluded to, requires for some of its further developments a standing point in 30° N. latitude.

Is the Great Pyramid, then, so placed?

It is, and it is not, I may perhaps be allowed to answer. At all events, it is *nearer* than any of the other pyramids; for, while the Aboo Roash abortion is several whole miles too far north, and the Great Pyramid (being, by my measures, in 29° 58′ 51″ latitude) is more than one mile too far south, all the other known pyramids are further south still.

Indeed, the exact position of the parallel of 30° latitude in that meridian is right down in the hollow alluvial plain in front, or northward, of the Great Pyramid; a region impossible for secure foundation. Rather, therefore, than incur that danger, would it not be better to put up with the error of 1′ 9″ in the angle of some of the lesser features of the building? Perhaps so; though meanwhile it may be doubted whether the error on the original intention really amounts even to the third part of 1′; for there are two ways of viewing the introduction of 30° latitude, or the symbolical elevation by so much of the polar point of the sky.

Shall it be, for instance, in a perfectly abstract manner, and as though the earth with all that is thereon were merely a mathematical point in space: or shall it be as the earth is in nature, where man can only view the polar star by gazing through the intervening medium of a refracting atmosphere, without which he could not live?

The former method would perhaps be preferred by the scientific few, but the latter is more adapted to the apprehensions of the greater number of the multitudes of mankind. Neither, therefore, can be altogether omitted in a monument both scientific and anthropological. Wherefore, as the former principle requires 30° 0′ 0″, and the latter 29° 58′ 23″ (or a latitude where the polar point, raised by refraction, would become 30° to the eye), we take a mean between the two, viz., 29° 59′ 12″, and find the Great Pyramid so very near thereto as necessarily to throw us back on that never-ending subject, the probable error of even the best modern determination; and also on that long felt physical doubt, as to the want of perfect constancy in relative position through long ages, between the crust of the earth and the direction of its axis of rotation.

But there is more important astronomy in the Great Pyramid than merely for geographical position.

The one only entrance passage, as already stated, looks out northward in the plane of the astronomical meridian and at the height almost of the polar point itself. Now certain well-meaning naval officers, about fifty years ago announced that, having descended that passage a certain distance at night, they saw, on looking up towards its mouth, the polar star, viz., *α* Ursæ Minoris, most clearly; whence they inferred, that the passage must have been built for the purpose of observing that same star in ancient times.

On this statement being submitted in 1839 by Col. Howard Vyse, when he had returned from North Africa, to Sir John Herschel, then just returned likewise from his celebrated astronomical sojourn in South Africa, he at once said,* These gentlemen have failed to take into account the *Precession of the Equinoxes*, which, in 4000 years, would have displaced every star in the heavens, from its then apparent position on the sphere by no less a quantity than 55° 45′ of longitude, and would have changed all the relations of the constellations to the diurnal sphere; whence it comes that the present pole star could by no possibility have been seen at any time in the twenty-four hours through the entrance passage into the Great Pyramid. But there was another star, he added, then almost exactly in that line, viz., *α* Draconis, and that must therefore have been the polar star of reference for the pyramid builders; the passage being built at such a vertical angle, in the meridian, as to point to that star at its transit beneath the pole, and at that precise date when Precession had made the star's distance therefrom to amount, for the time, to 3° 42′, or whatever the observed deficit of the passage angle below 30° might be.

This suggestion of Sir John Herschel's certainly introduced another, and in so far a dangerous, mode of explaining one passage angle which we have already got approximately from a very different consideration. In this latter case, however, the quantity was merely an auxiliary, while in Sir John Herschel's explanation we have the ultimate natural phenomenon itself. I freely, therefore, give his conclusion the preference; and even consider his contribution thereby to the astronomy of the Great Pyramid, as singularly important in its way, as John Taylor's π hypothesis has proved to be for the geometry.

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* See p. 107, vol. ii., of Vyse's "Pyramids of Gizeh."

And yet this pole-star-*cum*-Precession theory, though so admirably started, was eventually left by its brilliant author only half worked out. For α Draconis was not *once* only, but *twice*, in early history, at a distance of 3° 42′ from the polar point of the sky; viz., first in 3400 B.C. (nearly), and afterwards in 2170 B.C. (nearly). Wherefore all the world is surely entitled to ask, *which* of these two occasions was intended to be memorialised in the Great Pyramid?

At the time of Sir John Herschel's writing, viz., in 1839 A.D., a date of about 2100 B.C. was largely believed in as that in which the Great Pyramid was really built; so there seemed nothing to be done then, but to accept the *last* star distance, or that of 2170 B.C., as the one intended. But since then, learned Egyptologists have been so perseveringly stretching their early chronology, that the chief geniuses among them, such as Bunsen, Brugsch, Lepsius, Renan, and Mariette Bey, are all for dates of either 3400 B.C. or something earlier still. Whence it comes that the selfsame species of testimony which led Sir John Herschel in 1839 to decide for 2170 B.C., ought now to lead both him and everyone else also to prefer 3400 B.C.,—equally appealing all the time for proof to the same polar star, α Draconis, and the same entrance passage in the Great Pyramid.

Such, then, is the condition in which the brightest light of modern astronomy left this ancient question, or with an uncertainty of 1200 years (*i.e.*, almost a quarter of the whole time concerned) hanging over it. And if *that* were all the accuracy which the primeval architect cared to reach, I, for one, would cease to advocate his claims to attention from the learned in the present day. But let us try to inquire, and in something of that respectful frame of mind previously enlarged on as requisite, what *he*, the designer of the Great Pyramid, thought about this matter.

One of the very first features in Sir John Herschel's theory—which I, though beginning with the utmost desire to find it complete and faultless, could *not* admit; *i.e.*, after I had once acquired at the Pyramid by painful daily toil innumerable proofs of the wisdom, foresight, economy, and constant calculation of its primeval designer—was the attempt to father on *such* a man, and as a method in chronology, the taking a transit of a *polar-star!*

Why, does not the very youngest assistant in every modern observatory know, and know it as well as he knows black from white, that a *pole star* transit by itself is not the way to get an exact knowledge of the *time!* No, indeed, he seeks the very opposite, or an equatorial star, for such a purpose. And this broad principle is equally applicable to any tube, whether erected for actual observation, or merely for memorialising purposes, as was indeed the case in the Great Pyramid; where the passages, after being grandly constructed in such a totally diverse manner from the rest of the building, that they would be identifiable while any of it lasted—were immediately filled up with close fitting plugs of stone the whole way, and had their outer ends concealed until the days of dilapidation began.

Then, again, there chimed in with this pole-star-for-time difficulty—at least in my eyes and on the score of the respect due from every one living to the dead lion we were all standing upon—the further anomalous query, viz., that even *if* so bad a star for time purposes as a pole star *had* been adopted by the Pyramid architect, why did he take even that star in its weaker position, or at its lower, instead of its upper and brighter, culmination?

I do not pretend to say how other persons have contrived, with a clear conscience, to get over this double difficulty with merely an expression of contempt for all the knowledge of the best men in the world 4000 years ago, and the assertion that such fellows, barely developed in that early age out of primitive savagedom, were only thinking about "the best angle for sliding stones down the passage." But the rule of conduct which my Pyramid investigations had long since forced on me suggested, first the simple and very different answer,—"Because when that α Draconis star was crossing the meridian below the pole, another and more important star must have been crossing the same plane above the pole," and then set me inquiring about it.

Now the more important star for chronological purposes could be only an equatorial, or at least a zodiacal, star; a meridian combination of which, with a pole star for position reference, would be as powerful a method for fiducial time observation as any one could devise in the present day. But was there anything of the kind available at either of those two epochs, viz., 3400 B.C. or 2170 B.C.?

At 3400 B.C. there was no such combination, and α Draconis had to pass the meridian below the pole in barren state. But in 2170 B.C., see, and as the exact companion required, that zodiacal cluster, so dear to human sympathies throughout all post-diluvian ages, "the leader of the heavenly host" to the ancient Hindoos, and the type of Halcyon, *i.e.*, Alcyone-led, days to the classic peoples of Europe, viz., the Pleiades. Yes, indeed, no other star-group than the Pleiades, and at a distance *then*, as little removed from the equator as was α Draconis from the pole.

That typical polar distance has already been stated as 3° 42′; now the same star is 25° away; and the difference, or 21° 18′, exhibits something of the grand scale on which "Precession" allows the time elapsed to be computed; though for the further and best method we must rather look to what takes place simultaneously on the equator.

The moment, too, that we do look there, behold! there was something else on the meridian simultaneously with the Pleiades and α Draconis, viz., no less admirably suited and most rare a phenomenon,—for it occurs only once in 25,000 years,—than the vernal equinox. So that the Pyramid's precessional interval in chronology is measured off, on no accidental part of the general circumference, but from the very time-zero of the chief time-circle in the sky, viz., from one of the equator's intersections with the plane of the Ecliptic. 4040 years ago the stars of that vernal equinox, viz., then the Pleiades and their enclosing constellation Taurus, crossed the meridian at midnight, in the autumn portion of the year, say, on the 22nd of September. Now, the same stars are not seen in a similar position until the 19th of November: wherefore the number of days elapsed from Sept. 22nd to Nov. 19th, at the approximate precessional (really *re*cession of the equinoxes) rate, of 70 years to a day, gives at once the absolute date of the Great Pyramid we are searching for. The only absolute date, too, that has yet been discovered in any of the early monuments of the Egyptian land, and where, as well as in all that concerns the early history of man, it is so much required.

Most remarkably and completely, therefore, is the Great Pyramid, by the facts of its primitive construction, the special monument of "the Pleaides year;" wherein the year begins on that night when the Pleiades "are most visible," *i.e.*, when they are seen uninterruptedly from sun-set to sun-rise, and that is again, when they cross the meridian at midnight. An inimitable primeval method of measuring time over long intervals is thus procured, and which the researches, chiefly of Mr. R. G. Haliburton, of Halifax, Nova Scotia (its modern discoverer it may be said), have tended to show was universal among mankind before they left patriarchal rule and went off on their own inventions of "theotechny," war, chronology by their supposed even number of days in a year, and astronomy, not by the sure method of meridian passages, but by the fallacious risings and settings of stars. But this unexceptionable Pleiades and pyramid method, long running in men's minds, though ill understood, as among the ancient Romans,* is found even yet in chronological life and activity among the entirely unchanged representatives of extreme antiquity, such as Australians, and other stationary races: in these days, invaluable representatives; for they have never, since their first supranatural dispersion, had the then imparted mental heirlooms of their race interfered with by the substitutions of other men's progressive development, and the artificial wonders,—changing from age to age like the patterns of a kaleidoscope,—of their school civilisation.

5. *The Interior of the Great Pyramid in its relation to the Interior of the Earth.*

When studying the *linear* dimensions of the Great Pyramid at p. 199, we found its one, all compelling feature for whole size, viz., vertical height, alluding in even terms of the Pyramid numbers $10^{3\times3}$ to that much-meaning symbol of *external* creation, the sun's mean distance from the earth.

But if we now commence an enquiry touching the next chief feature of physical description after size, viz., *weight*, our ideas are immediately turned towards our own *earth*, and there to its *interior;* which, if less extensive, is a vastly more mysterious region than the exterior creation around it. For the distant sun, in spite of his distance, we may both see and optically measure,—but who can look through the solid depths of the earth, right down to the very centre, and tell all the various products of which its mighty opaque mass is composed, and on whose multifarious chemical qualities the chief success of modern civilisation depends?

No man, of course, can do so, or knows, or need ever expect to know, how much gold, silver, iron, and other important elementary substances there may be still stored away within "the deep of the earth that lieth under." But taking the question in another way,—scientific measure

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* "Candidus auratis aperit cum cornibus annum Taurus."—*Virgil.* A passage on which many commentators have long since remarked, "that it does not apply with astronomical truth to Virgil's own day, but to a period two thousand years earlier:" though why Virgil should have gone back to that previous time they had no idea.

can approximate to the *mean specific gravity* of the whole contents of the earth taken faithfully and on trust together; and can say with confidence, that the all-important quantity in physics thence arising, is close to 5·7 times the specific gravity of that ever present, almost universal, surface fluid of the earth, water. Whence, having already from the linear measures of modern science, the outside size and shape of the globe, we may readily compute its weight (here in a particular species of tons), and shall find it to amount to 5,271,900,000,000,000,000; (the numbers, however, not being trustworthy to more than the three, possibly four, first figures, and the remaining 0's indicating uncertainty rather than exactness, in everything beyond "place.")

Now, between the *bulk* of the earth and the bulk of the Great Pyramid, there is no even numerical relation. Nor should there be, if the size has been settled upon a different feature of the Kosmos. But between the above *weight* of the earth and the *weight* of the Pyramid there may, or may not, be something of the kind; because, without in the slightest degree interfering with shape, size, or astronomical position, the architect might use a material of any given density.

Now, the substance which that primeval designer did choose, is very peculiar in respect of weight for a given bulk. For whereas, in terms of the earth-globe as unity, the specific gravity of granite is 0·479, and of what is usually called by builders in this country "common stone," 0·442, —that of the chief material of the Great Pyramid is only 0·412, and a portion so low as 0·367. Whence, calculating the separate weights of each portion, we obtain for the final sum of the Pyramid's weight (in terms of the same tons as before), 5,273,000 nearly;* or a quantity which may be practically regarded as being to the whole earth's weight, in the grandly round, even, and truly Great Pyramid proportion of $1 : 10^{5\times3}$.

The *linear* relations, however, of the Great Pyramid did not stop at a single cosmical equivalent of the whole, but proceeded, after the introduction of a subsidiary idea, to another and more practical relation on a very much smaller scale, and more fitted for everyday use among men. Is there, then, anything of the same kind in the *weight* relation of the Great Pyramid?

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*See "Antiquity of Intellectual Man," by C. Piazzi Smyth, p. 475.

There is, indeed; and though the instrument thereof be shrouded in the eternal darkness of the most central interior, yet it signifies far and wide its ruling importance by spreading a certain type of its nature throughout the entire substance of the whole building, even to its very exterior, and equally on the four sides; accomplishing it thus.

In the highest, innermost, most granite-built, most regularly fashioned, and final chamber of the Great Pyramid, there is a long, hollow, lidless, rectangular box admirably carved out of a single block of hard red granite. It is the only piece of furniture in the room, and is quite loose, or stands freely, and may be pushed about hither and thither on the flat granite floor; and has been known to the world sometimes as a "sarcophagus" for burial purposes, and sometimes as a "vase," "coffer," "laver," "font;" and at last as a grand standard of capacity and weight measure,—the cubic contents of the interior showing the former, and the weight of water, at a certain temperature, required to fill it, the latter. John Taylor, too, went even a step further than his predecessors, in proving that the contents of the vessel were equal to those of the *laver* of the Hebrews, and its 4th part the same as the so-called *quarter* of early Saxon metrology,—but a quarter of *what* the oldest existing tradition had allowed to escape.

There is much in all this requiring independent mechanical proof before its full force can be appreciated.

First, for instance, if the vessel be loose, and the door high enough to allow of its passing in and out (and as the said door does by just a fraction of an inch), how are we to feel assured that such a portable thing has not been brought in at some long subsequent time, and not by the original builders?

In this way:—

The height of said coffer is to the length of two sides of its base as 1 to 3·141, &c.,[*] *i.e.*, it is a π proportioned coffer in a π proportioned Pyramid; a shape which applies to the Great Pyramid alone: while further still, the cubic contents of the interior, most carefully measured,[†] are 1-50th of those of an anciently and accurately marked off portion of the lower granite course of the chamber, itself containing the said coffer. That vessel, therefore, being found,—when tested by ideas which were never ventilated until the last few years,—to be so eminently appropriate in shape, as

[*] See St. John V. Day's "Papers on the Great Pyramid," p. —; also p. 13 of his "Plates and Notes on Structures called Pyramids:" Edmonston and Douglas, Edinburgh.

[†] See "Life and Work," vol. iii., p. 154 and 167.

well as size, to the very chamber and of the identical Pyramid in which it stands—cannot be other than the very original vessel for which the chamber was anciently built.

And what was it introduced there for?

"To serve as a sarcophagus," says the general Egyptological world. Wherefore, as we have always shown gentlemen of that creed so much respect, let us seriously examine their idea in this case also.

The sides of a granite sarcophagus in ancient Egypt were just the very places of all others, *teste* Sir Gardner Wilkinson, where the prospective occupier,—if rich enough,—or his friends after him, invariably caused his name, titles, wealth, and glory to be most signally and abundantly engraved. But there is not a shadow of carving, painting, or writing here, on what some persons will persist in calling "King Cheops's coffin." *

Again, *if* a mummied corpse used to be deposited in its sarcophagus at the usual place of abode amonst the living, and the lid of said vessel then fastened on securely before the transport to the tomb began,—such lid, if made according to any and every example of the period,† would add so many inches to the height of the *coffer of the Great Pyramid* (which is already higher than any known sarcophagus of the Old Empire), as totally to prevent it getting into that building at all, much less coming through the low doorway of its so-called King's Chamber.

There is, indeed, a certain partial ledge cut out in the top of the sides of the coffer, *almost* as if for fastening on a lid; but it would not have held, if tried; and is found, on measurement being applied, to supply a peculiar and, in the present day of mischievous dilapidations by travellers, a very much needed confirmation of the original full *depth* measure of the coffer. In fact, the whole structure teems, not with mortuary remains, but with practical geometry: for, in addition to the above, the thickness of sides and bottom have been made such, that the cubic contents of the bottom are half those of the sides; and the cubic content of both sides and bottom together are equal to that of the interior;* or the interior is just half the exterior. These proportions, however, are only obtained by theoretically filling up the said "ledge cut out;" but being so exactly obtained then, prove the said ledge to be adventitious. Taking the coffer, then, without that later infraction into its integrity of figure,—let us enquire, what determined its most important feature,—if a measurer of capacity—and that by which it is so largely distinguished from *bonâ fide* sarcophagi from other pyramids and neighbouring tombs,—viz., its *internal cubic contents?*

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* Though that King did not glorify *himself* by inscriptions either on the Coffer, or any of the finished surfaces of the vast monument in which it stands,—yet his name and memory have not perished amongst men, as some of the detracting class are fond of insisting on, when denouncing the building of the Great Pyramid as a piece of sheer barbarism and utter folly. For his true name, in the original Egyptian form or *Shofo*, was discovered by Colonel Howard Vyse when he excavated his way into certain hollow spaces of masonry above the King's Chamber, and found it painted on some of the stones for quarry marks, evidently by the working men of the period: a discovery which at once vivified and gave tangible substance to the Grecian form preserved in Herodotus, viz., *Cheops*.

True, indeed, that the name is preserved there, only as accompanied by a most odious tradition as to moral character; but that, as John Taylor showed, is rather to be taken as a measure of the *purity* of that King's religion, whose chief end and aim, according even to his enemies, was to root out idolatry from the land. And that beginning of the removal of the aspersions of 4000 years from the memory of one who worked not for himself but for God,—may, D.V., go on increasing until King Shofo shall become one of the best known names and most highly appreciated characters in primeval history.

† See the oldest granite sarcophagi at the Museum of the Egyptian Government at Boolak.

This. The 10^7th part of the earth's axis of rotation—cubed to represent solidity,—and then multiplied by the earth's mean specific gravity 5·7, divided by 10.

Now such 10^7th part amounts to 50 of the same Pyramid units, whereof we took 25 in the linear investigations: wherefore—

$$50^3 \times \tfrac{5{\cdot}7}{10} = 71{,}250.$$

or as close as the best measures in the same units have yet made the cubic contents of the coffer to be, or rather to have been, anciently. While again—

$$50^3 \times 5{\cdot}7 \times 5 = 3{,}562{,}500,$$

or the cubic contents, within a few inches, similarly measured, of the marked off portion of the lowest granite course of the chamber walls holding the said coffer.

The number 50, then, plays a very important part in this investigation; and appears here first, in terms of previously known linear Pyramid units, whereof 500 millions make the length of the whole axis of rotation of the earth. Now the chamber itself contains, or is made up of, as to its walls, 5 grand, even, regular courses of polished red granite, which courses run round and round the room in uninterrupted horizontal lines of remarkable mechanical perfection,—while the physical nature of their surfaces, glittering, characteristically for granite, with innumerable distinct and parti-coloured little crystals, beyond any other species of stone there, might be taken almost as emblematic of some mighty and extensive number, though not necessarily 100,000,000. But this is only a beginning; for next, in a far more remarkable and certain manner, the floor of this chamber (on which the coffer, so eminently depending on the standard of 50 of those units long, actually stands) is found to be coincident with the 50th course of colossal, wide-spread, horizontal masonry which forms the structure of the entire Pyramid:* and marks off outside, but for those only who know them by measure, the levels of the scientific chambers within.

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* See "Antiquity of Intellectual Man," pp. 295 to 301; also Mr. Henry Perigal's published "Original Dimensions of the Coffer."

Plainly, therefore, by this constantly increasing number of consilient and allied coincidences, we are dealing here with numbers and proportions arising from no mere accident. And when it has been still further and variously shown that the cubic contents of this unique Great Pyramid coffer were precisely those of the most sacred "Ark of the Covenant" of Moses, under a symbology, too, also indicating the solid earth as a whole: and when the marked portion of the chamber course is found equal in cubic contents to Solomon's Molten Sea; † and when the exact cubic contents of one-fourth of the coffer,—or equally the Hebrew chomer and Anglo-Saxon quarter,—have been found more recently ‡ to be expressed by the lower stone of that granite leaf, of which the upper one expresses the division of the sacred and Pyramid cubit into 5 and 5 again, or into the very units employed—we are plainly on a threshold where the primeval architect drew from the same eternal well of truth as did Noah, Moses, and David at almost equal intervals with him, through the ages; and where the material in which this first of architects in stone worked, has enabled his own *ipsissima verba*, as it were, to be preserved for our reading, testing, and guidance down to this very day. And if so, to raise this still further question above even all others, viz., if or rather when all these things were thus evidently "prepared from the beginning of the world," why have they been allowed only just now to be in part rightly apprehended?

Perfectly inexcusable, however, would it be in me, at this most extended point of a repeated paper, to crave a little more, and then, doubtless, more space still after that from our most complaisant and intelligent Editor; for there *now* rises in view really only the beginning of the end; or a vast, unsounded subject, and which, when it comes to be fairly worked out in its several applications to anthropology, past, present, and future, can hardly fail to transcend infinitely all that mere physical science will ever be able to accomplish for the final amelioration of the human race in its higher attributes and ultimate destinations. Yet is it only by physical science rightly applied that this ancient and mighty message of old, to the present day, can be read.

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* See "Life and Work," vol. iii, p. 172.
† See "Life and Work," vol. ii, p. 466.
‡ By Captain Tracey, R.A.

What crusade, then, is there of our times so important as, that of, or for, a correct, complete, and exact Great Pyramid investigation at the place? A few individuals—and by no means rich ones—have done their little in the cause most heartily: but the next steps are for kings, princes, and nations; and oh! that *they* would heedfully bear in mind that it is written,—"It is the glory of God to conceal a thing; but the honour of kings is to search out a matter."

V. STEAM BOILER LEGISLATION.

By Sir William Fairbairn, Bart., LL.D., F.R.S.

A QUESTION of great public interest has arisen touching the prevention of Steam Boiler Explosions. The desire for increased pressures of steam in order to save fuel, has led to steam boilers in many cases being taxed beyond their strength; and, as under the present system of obtaining motive power, these important vessels cannot be dispensed with, it has become necessary that some steps should be taken to protect her Majesty's subjects from the danger both to life and limb to which they are now constantly exposed. The number of fatal explosions which occur from year to year, and from month to month,—one explosion taking place every week, and one life being sacrificed every fourth working day in the year,—has called forth a spirit of inquiry into the causes of these unfortunate catastrophes, and to a certain extent has led to the adoption of preventive measures.

The Manchester Steam Users' Association was founded 15 years ago under the original title of "The Association for the Prevention of Steam Boiler Explosions, and for effecting Economy in the Raising and Use of Steam." It was established by a few philanthropic and disinterested gentlemen connected with manufactures, and carried out by the writer and owners of mills for self protection, and the reduction of those calamitous events, steam boiler explosions.

Such was the origin of the Association. It has ever since been in active operation, and while increasing in the number of its members, has proved not only of advantage to them but to the public generally. The investigations and experiments conducted by the Committee, have also conduced to improved forms and secure principles on which boilers should be constructed.

After the above Association had been in work for some years, several joint stock companies were established for carrying out inspection accompanied with insurance. It will be readily understood, however, that the object of a joint stock company must be the promotion of dividend; and as it has been proved by experience that inspection is very costly, while insurance is very cheap, it is evident that the introduction of the joint stock principle will militate against the efficiency of the inspections, and that it will tend to promote insurance rather than inspection. Insurance of boilers will not restore life after a fatal explosion has occurred, while the companies make no provision for the families and representatives of those killed and injured. They profess only to compensate the owner of the boiler, though it may have been old, worn out, and quite unfit for use, while they do nothing for the poor stokers who may have been killed or injured, or for their families who may have lost their bread-winners. It is not intended by these remarks by any means to condemn the principle of insurance generally, as it may advantageously be adopted as a provision against the loss of shipping at sea, the destruction of property by fire, or in the event of death—whether accidental or otherwise,—certain sums having been paid for the securities offered. This system of insurance is satisfactory where no inspection is required, but it is not satisfactory in its application to boilers with the view of preventing explosions, since this end can only be obtained by constant and costly inspection, which the demand for dividend on the part of the shareholders will tend to diminish to the lowest possible point. It is on these grounds that inspection carried out by joint stock insurance companies cannot admit of comparison with that of voluntary honorary Associations, whose sole object is—careful inspection, for the prevention of steam boiler explosions, and the saving of human life.

During the early days of the steam engine it was considered unsafe to work steam at more than 7 lbs. on the square inch, and no difficulty existed in making boilers to sustain that pressure. The first in respect of form was what was called the Haycock boiler; next came the waggon boiler, by Watt, which was the favourite for more than forty years; again followed the circular Cornish boiler used for pumping engines, which for a number of years was worked at a pressure of 30 to 40 lbs. on the square inch. These were the pressures at which Cornish boilers were worked forty years ago, but the extension of trade, and the demand for increased pressure and increased power have produced great changes. It was found that, in order to make an engine do double the work, it was only necessary to double the pressure and increase the velocity or number of strokes until the required force was obtained. This was done with great advantage as regards a saving of fuel, but not without risk; and thence ensued a continued series of break downs and boiler explosions. These at one time became so frequent that the writer was for several years summoned by coroners' juries to investigate the causes of these disasters, and to suggest remedies such as appeared necessary to reduce the risk, and, if possible, to prevent the lamentable results of boiler explosions. These enquiries ultimately led to the formation of the Association above described, which since that time to the present has been of great benefit to the public.

From the greatly increased and still increasing pressure of steam at which engines are worked will be seen the importance of having perfectly secure and suitable vessels to contain a force of so destructive a power as steam. It does not appear necessary to adopt a retrograde process, and reduce the pressure to 7 or 10 lbs. as that of former days. On the contrary, it is desirable to prepare the public for a still further increase of the working pressure if we are to secure a satisfactory result of economy in the use of fuel.

All these elements connected with steam, will, therefore, have to be carefully considered in order to acquire a motive power calculated to cheapen production in every stage of our manufacturing process. This, it will be observed, can be accomplished with perfect safety provided that boilers are made on sound principles of construction, and accompanied with the use of sound material and good workmanship. It must be borne in mind that the practice of boiler construction, which at present exist cannot, in many cases, be too severely censured, as these constructions demands the skill of our best engineers, and the exercise of definite laws founded on experimental research.*

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* It not unfrequently happens that some of the mill owners in the manufacturing districts are their own engineers, in which they frequently stipulate for having their own proportions of steam engines, such as the diameters of the

It has been stated, and that correctly, that boiler explosions may be reduced to a minimum, if not entirely prevented, by competent periodical inspection; and since steam users neglect so simple a precaution, and persist in sacrificing 75 lives every year, it must clearly be the duty of the Government to interfere on behalf of those whose lives are jeopardised, and to enact that no boiler shall be worked unless periodically examined and certified. To arrive at the best plan, however, of administering this inspection so as to prevent explosions on the one hand, without unduly interfering with the liberty of the steam user on the other, is a problem of some difficulty. It received the most careful attention of the Committee of the Manchester Steam Users' Association, and after much consideration they drew up a report on the subject, giving the conclusions at which the Association had arrived after fifteen years' experience. These conclusions are as follows :—

Proposition No. 1.

That the use of steam, &c., as at present conducted, entails great suffering from the destruction of life and property occasioned by the constant recurrence of boiler explosions.

That boilers are now to be found under the pavements over which the public walk, behind walls close to which they pass, in the basement of buildings crowded with busy workpeople; and, that, in short, they are to be found everywhere. That many of such boilers have given rise to the most disastrous explosions, so that the lives of all those living near so dangerous an instrument as a boiler, or even casually passing by, are seriously jeopardised unless suitable precautions are adopted to ascertain whether the boiler be safe and trustworthy; and, if not, to render it so. That most of those who have suffered from these explosions have had no voice in the management of the boilers, and thus were helplessly victimised, some being women in their own houses, and others, children at play. Further, that in the generality of cases, those injured by the explosions of boilers at the works at which they earn their livelihood are in a similarly helpless position.

That the subject, therefore, becomes one of general and public interest demanding immediate investigation, more especially since the use of steam is daily on the increase,

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cylinder, air pump, length of stroke, &c. They also in some cases stipulate for the dimension, form, and construction of boilers; but it must be admitted that in many instances their knowledge is not inferior, and in some cases superior, to the engineer they employ.

and notwithstanding any precautionary measures at present adopted, explosions still recur with the most persistent regularity and frequency.

The following list exhibits the numbers of killed and injured during the last 16 years.

Table showing the Number of Explosions Recorded by the Manchester Steam Users' Association from the commencement of 1855 *to the* 31*st of December,* 1870.

Year.	Number of explosions.	Persons killed.	Persons injured.	Total.
1855	10	10	1	11
1856	31	58	70	128
1857	27	67	58	125
1858	33	78	100	178
1859	26	57	65	122
1860	22	45	36	81
1861	22	30	51	81
1862	28	87	89	176
1863	48	76	80	156
1864	33	65	91	156
1865	48	46	79	125
1866	72	87	108	195
1867	36	60	67	127
1868	45	57	60	117
1869	58	86	126	212
1870	51	78	106	184
Total, 16 Years	590	987	1187	2174

From this Table it will be seen that 590 explosions have occurred since the foundation of the Association, killing 987 persons, and injuring 1187 others, while to this it may be added, that many thousand pounds' worth of property has been destroyed. This return, however, does not include the total number of explosions, or the total number of persons killed and injured during the period referred to, since in the early years of the Association's operations such complete records were not kept as is now the case, so that many escaped its notice. At the present time there occur on an average 50 explosions a year, killing about 75 persons and injuring as many others, so that an explosion occurs every week, and one person is killed every fourth working day in the year.

Proposition No. 2.

That boiler explosions are not a necessary consequence of the use of steam, but that they are preventable. That though complicated in result, they are simple in cause; arising in the main from bad boilers, bad either in construction, or bad in condition. That six explosions are due to bad boilers, through the neglect of the boiler maker or boiler master, for every one due to the neglect of the boiler minder. That competent inspection is adequate to detect the badness of the boilers, and thus to prevent by far the greater number of the explosions now occurring.

In support of the statement that explosions arise from simple and preventable causes, the following Table may be adduced.

Table showing the Causes of the Steam Boiler Explosions that occurred from the 1st *of January,* 1861, *to the* 18th *of June,* 1870.

Cause of explosions.	Percentage of whole number.	Number. of explosions.	Persons killed.	Persons injured.
Malconstruction of the boilers, either in the shells or fittings	40	120	216	268
Defective condition, either in the shells or fittings	29	88	188	239
Failure of the seams of rivets at the bottom of externally fired boilers	15	44	64	67
Overheating of the plates from shortness of water	10	30	23	23
Overheating of the plates	—	—	—	—
From incrustation.. .. .. (6)	3	8	14	12
From the use of boiler compositions .. .. (1)				
From causes requiring further investigation .. (1)				
Excessive pressure through attendants tampering with safety-valves	2	5	10	12
Explosion of economiser, but whether from gas or overpressure of steam is uncertain	½	1	—	—
Cause entirely independent of the condition or construction of the boiler, and may be termed accidental	½	1	1	1
Number of cases in which the cause has been investigated	100	297	516	622
Number of cases in which the cause could not be investigated for want of sufficient particulars	—	114	123	160
Total number of explosions	—	411	639	782

In the records which the Association has filed, minute particulars and illustrations are detailed of the majority of the explosions narrated in the above Table. From this it will be seen that the greater number of the whole arose from malconstruction and defective condition, the danger arising from which competent inspection is able to detect in time to prevent explosion. It will also be seen that adding the number of explosions that arose from overheating of the plates, whether from shortness of water, or from incrustation to those arising from the attendants tampering with the safety-valves and attributing them all to the neglect of the boiler minder, that they do not exceed 15 per cent of the whole number,—or, in round numbers, one in seven, so that, as stated above, where the stoker or boiler minder has to answer for one explosion, the boiler master and boiler maker have to answer for six. It is important that this should be kept in view, as the attempt is constantly made to attribute the majority of explosions to the fault of the boiler minder, and thus to show that, after all, inspection is unable to grapple with steam boiler explosions. A review, however, of the facts of the case shows how ill founded these assertions are.

To this it should be added, in justice to the poor boiler minder, so often abused, that it is by no means clear that he should be made accountable for the whole of the 15 per cent of the explosions referred to above, and they were only tacitly admitted in the course of argument that it might be apparent that the case in his favour was not over-stated but under-stated. In many of the cases of explosions from shortness of water, the boilers have been lamentably deficient of suitable mountings, a condition for which the engineer and employer are exclusively responsible.

Proposition No. 3.

That notwithstanding the proved efficiency of competent boiler inspection, and the publicity constantly given to the subject, yet that steam users refuse to protect the lives of their workpeople, or those residing near to their works, by having their boilers inspected. That it appears approximately that out of more than 100,000 boilers in the country, only 20,000 are enrolled either with inspecting associations or insurance companies, so that out of every five boilers one only is enrolled. That a great number of boiler owners are totally ignorant of the risk to which they expose their own lives and those around them, and in many cases are only undeceived by the shock of explosion. That, judging from experience, there can be no doubt that there are now a number of dangerous boilers on the very verge of explosion being worked on at the risk of all those living near them. That under these circumstances the public safety demands that competent periodical inspection should be enforced by law, and that it should be rendered as illegal for the lives of the public to be jeopardised by dangerous boilers as by the storage of gunpowder, petroleum, or other explosive materials.

The committee have most reluctantly arrived at the conclusion that it is necessary to enforce inspection by law, but the inexorable logic of facts has left them no alternative.

This Association, as already stated above, was founded fifteen years ago for the administration of a system of voluntary periodical inspection with a view to the prevention of steam boiler explosions. The experience of that term of years has shown that inspection is adequate, but that voluntaryism is not. Inspection has succeeded, voluntaryism has failed. The difficulty is not with the boilers, but with the boiler owners. Inspection will prevent explosions, but the owners will not have their boilers inspected.

The cause of this neglect of inspection may in many instances be simple ignorance; and it is thought that the recent explosion at Liverpool may be taken as a representative one. Here was an old second-hand worn-out boiler eaten away by corrosion till reduced to about a sixteenth of an inch in thickness, yet worked at a pressure of 60 or 70 lbs. on the square inch. It was situated in a populous part of the town, and surrounded with dwelling-houses. It was only cut off from a public thoroughfare by a 9-inch wall, which afforded concealment but no protection. The boiler exploded; the sanctity of the dwelling-houses all around was invaded; the greater part of the boiler was hurled into one house, and a fragment into another, while the ruins of the boiler setting and other *débris* were scattered in every direction. Two children in one house were killed, and a woman injured; another child was killed in the public street, and a lad at the works scalded and crushed to death. Yet the owners appear to have been ignorant of the danger. The boiler had but a few months before received a home-spun inspection, and been declared perfectly sound, and equal to last the owners' lifetime. The owners trusted it; both of them were within a few yards of the boiler at the moment it burst, and narrowly escaped being killed themselves. The destruction of their premises exhausted their resources, and thus left them unable to repair the ruin their bad boiler had caused. The public outside had no voice in the management of this boiler, yet they had to suffer the consequences of its explosion. All this suffering may be fairly attributed to ignorance on the part of the owners, or they would not have jeopardised their own lives in the manner they did. Nothing but coercion in some form or other will meet such cases as this.

Proposition No. 4.

That although it is necessary in the interest of the public that inspection should be enforced by law, it is not advisable, either in the interest of the steam user or the public at large, that that inspection should be undertaken by the Board of Trade or any other department of the Imperial Government, as such a course would, it is feared, be a source of annoyance to the steam user, and hamper progress.

The above proposition needs but little to be said in its support, as the view is so generally entertained that an enforced system of inspection administered by the Government would be irksome and obstructive. Without any disrespect to the Board of Trade or any other department of the Government, it is thought that such a system would have a tendency to be always behind the times. As improvements were made from day to day, the system would require to be constantly modified, which it is thought with a Governmental administration it would be difficult to accomplish.

Proposition No. 5.

That while the administration of a system of enforced inspection should not be committed to the Imperial Government, neither should it be committed to municipal governments, nor to private inspecting associations, insurance companies, boiler makers, or others, as there would then be no guarantee for the virtue of the inspection. That conducted by town councils would be liable to be biassed by party interests, while the practice of one municipality would vary from and contradict that of another. That conducted by private associations and companies, and more especially by private individuals, would be irresponsible; and as those who gave the greatest license would get the greatest number of fees, the certificates would soon become worthless and degenerate into a sale of indulgences, so as utterly to defeat the object in view of promoting the public safety.

In support of the proposition that the practice of one municipality would vary from and contradict that of another, a brief extract may be given from the report presented to the last meeting of the British Association for the Advancement of Science on the subject of Steam Boiler Explosion Legislation. "The science of boiler making is progressive; it is in a transition state, and in spite of the amount of information constantly disseminated, great ignorance prevails with regard to it. In consequence of this, one corporation would declare a boiler safe which another corporation would declare unsafe; so that a boiler carried by rail from one part of the country to another might be counted safe at the beginning of its journey, and unsafe at the end. For instance, in Lancashire, the practice of strengthening flue tubes at the ring seams, with flanged joints, or hoops of T iron, or other suitable section is highly approved.* In fact, it is thought that no high-pressure boiler should be constructed now-a-days without these appliances. In Cornwall, however, nothing can convince steam users of their necessity, and Cornishmen persistently adhere to the idea which the Franklin Institute of Pennsylvania endeavoured to dispel thirty-four years ago, viz., that a boiler cannot explode as long as it is properly supplied with water. They appear to believe that furnace tubes, though of great length and diameter, and though worked at high pressures of steam, can only collapse from the neglect of the water supply, or in other words, from the neglect of the attendant, and not of the owner or the maker. In Cornwall, boiler flue after boiler flue collapses, simply from weakness, till the Cornish boiler stands in the return of explosions as one of the most dangerous."

Proposition No. 6.

That while it is advisable that inspection should be enforced by law, though not administered by the Government, whether imperial or local, nor by private inspecting associations, insurance companies, or boiler makers, lest the administration should be arbitrary in some cases and contradictory and inefficient in others, yet that a system of administration, competent to secure the interests of the public on the one hand, without detriment to the steam users on the other, might be framed on the following basis:—

To secure the efficiency of the inspection, let the administration be national, so as to be above all local party or private interests, and let it be undertaken not for profit, but to promote the public safety. To prevent the administration becoming arbitrary, stereotyped, and old-fashioned, and to render it capable of adaptation to the constantly-altering and growing requirements of the boiler owner, let it be administered by a series of district Boards elected by the steam users themselves; the Boards having the power of making such laws, rules, and regulations from time to time as might be found necessary for conducting the service. Such a mode of administration would, it is thought, not only secure efficient inspection adequate to prevent explosions, but also be considerate to individual steam-users, and be found not to hinder but to assist engineering progress. On this system the inspection would be enforced by law to render its adoption universal; it would be administered by the steam-users themselves to prevent its becoming arbitrary; and, finally, it would be founded on a national basis to secure its accomplishment.*

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* For the experiments connected with this subject—*the Collapse of Tubes*—see "Philosophical Transactions."

Such are the conclusions at which the Manchester Steam Users' Association has arrived after mature consideration. It will be understood that the district Boards proposed, are to cover, in their operations, the entire country. Also, that they are to be of mixed constitution, most of the members being men of commerce, *i.e.*, employing steam power for mercantile purposes, and others, men of science, *i.e.*, engineers, chemists, and others, competent to advise on matters affecting the inspection of boilers, or the interests of steam users generally, to assist at the counsels of the Board, and to add weight to their decisions. These Boards to be empowered to impose such a boiler-rate as might be found necessary to ensure competent and thorough inspection. Monthly Reports very similar to those of the Manchester Steam Users' Association to be issued, these reports giving the number of inspections, the state of the boilers, &c. It is further proposed that the monthly reports, or the substance of them, should form the basis of an annual report, to be presented to the members of both Houses of Parliament.

On this plan, it is thought that a system of periodical inspection would be insured, which would be competent to prevent steam boiler explosions, and thus save the lives now sacrificed, without in any way incurring unnecessary interference with the steam user, but protect alike the interests of manufacturers and the public. Ultimately it would prove a help and not a hindrance to scientific and commercial progress.

On further inquiry, other suggestions and improvements will no doubt present themselves which might be adopted without detriment to the fundamental principles of the system proposed, which are, firstly—that steam boiler inspection should be rendered compulsory, and, secondly, that it be allowed to be administered by a national series of district Steam Boards, appointed in the main, if not entirely, by steam users themselves.

It may be interesting to append, in illustration of the foregoing remarks, an engraved photographic view of the

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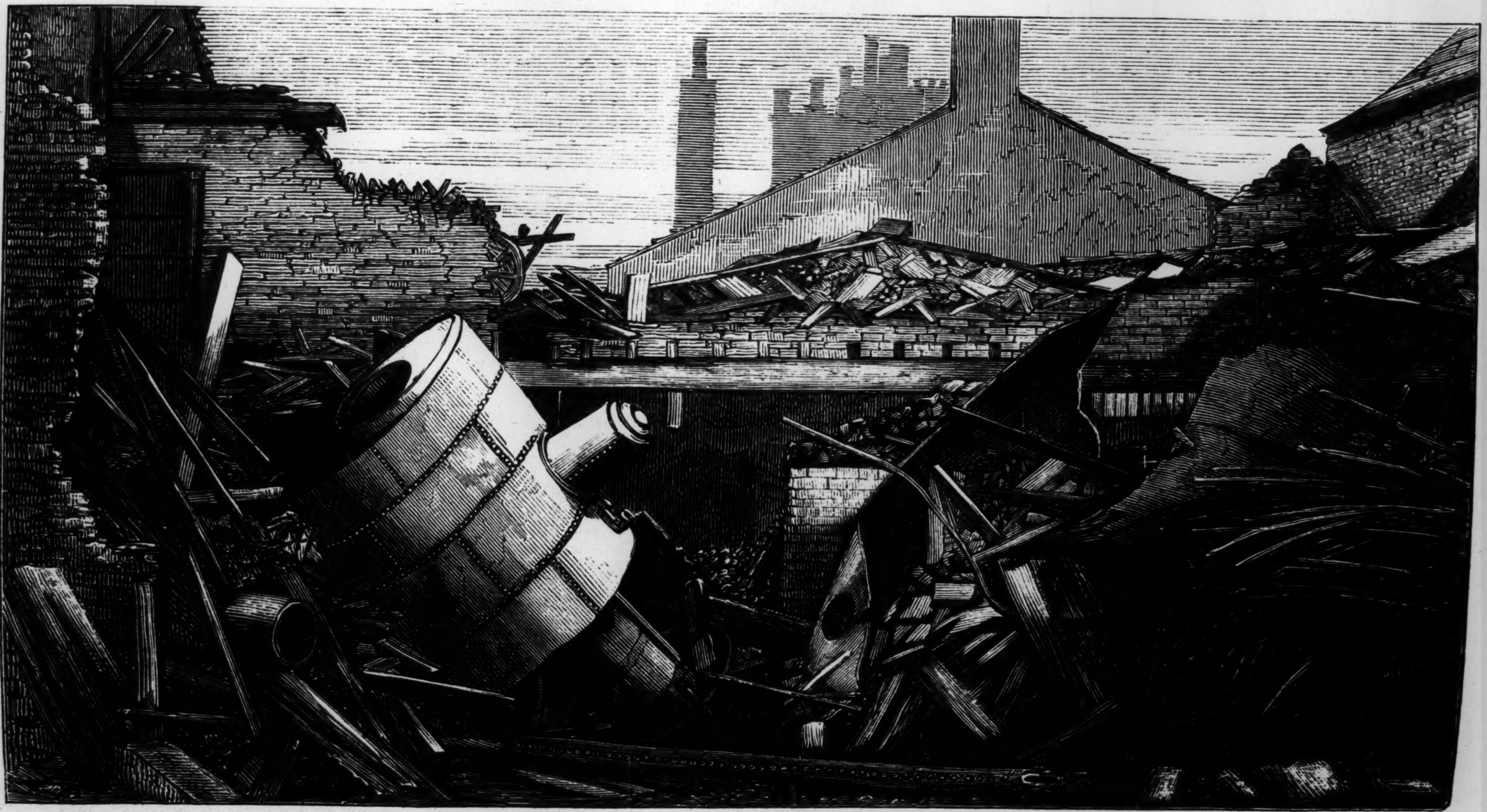
*** For somewhat fuller details of the proposed National Steam Boards, see Report on "Steam Boiler Explosion Legislation," presented to the last meeting of the British Association for the Advancement of Science.**

FIG. 8.



EXPLODED BOILER NOT UNDER THE INSPECTION OF THE MANCHESTER STEAM USERS' ASSOCIATION.

The Explosion occurred at Messrs. Chapman and Holland's, Ashley Lane, Manchester, on Monday, December 23rd, 1867. Six persons killed, and four others injured.

destructive effects of boiler explosions. It is taken from some hundreds in the possession of the "Steam Users' Association for the Prevention of Boiler Explosions." It does not exhibit one of the most fatal and disastrous occurrences to life and property as some others; but the catastrophe is sufficient to claim the sympathies of the public, and to enlist the interference of the Government, and that more particularly when we are assured that immunity from these disasters may be obtained from careful periodical inspection. It is not high pressure steam that we have to fear, but unsuitable vessels, designed and constructed by ignorance, and want of proper inspection we have to guard against. These defects remedied, by sound principles of construction in the first instance, and careful inspection in the second, would go far to make boiler explosion the exception and not the rule.

"This explosion, which took place a few minutes after one o'clock on the afternoon of Monday, December 23rd, was of a very disastrous character, as many as six persons being killed, and four others injured, while the dye works at which it occurred were levelled to the ground.

"The boiler, which was set immediately under the drying room, was of the ordinary Cornish construction, being internally fired, and containing a single furnace tube. Its length was 18 feet, its diameter in the shell 6 feet, and in the furnace tube about 3 feet 2 inches, while the thickness of the plates was three-eighths of an inch, and the blowing-off pressure about 25 lbs. on the square inch.

"The boiler failed at the bottom of the external shell, rending longitudinally from one end to the other, when the whole was lifted from its seat, and the entire works laid in ruins. It was to this that the loss of so many lives was due, and not to scalding, or injuries received directly from the boiler, but to the fall of the works upon the poor fellows who were crushed and buried in the ruins.

"The cause of the explosion was but too apparent on examination. The boiler, which was set on a midfeather wall, had been shamefully neglected; the external brickwork flues were very damp, leakage had occurred at many of the seams, the boiler had not been properly examined, and external corrosion had been allowed to go on eating into the plates till they were wasted away at the bottom of the shell to the thickness of a sheet of brown paper, almost from one end of the boiler to the other. The boiler was totally unfit for work, and it is a matter of surprise how it withstood the steam pressure at all. The explosion is a striking instance of the folly of neglecting periodical inspection.

"At the coroner's inquest the jury returned a verdict of 'Accidental Death,' adding that they considered great neglect had been displayed by the conduct of the employers, and urging the coroner to call the attention of the Home Secretary to the fact that it is considered desirable that the periodical inspection of boilers should be compulsory."

VI. THE ECLIPSE OF LAST DECEMBER,

WITH SPECIAL REFERENCE TO THE INTERPRETATION OF THE SOLAR CORONA.

By Richard A. Proctor, B.A., Cambridge.

SIX months ago, I dealt in these pages with the circumstances of the eclipse to which astronomers were at that time looking hopefully forward. The plate which illustrated my paper showed the course which would be followed by the moon's shadow as it swept across the earth's surface. In that plate, the chief observing stations were also indicated; while in the body of the paper I discussed the modes of observation which astronomers and physicists thought of applying. I have now to exhibit the results which have rewarded the exertions of those who have taken part in the various expeditions.

There were so many observers, however, and the reports already to hand are so voluminous, that it seems advisable to make a selection of those results only which throw a new light on problems of solar physics. Indeed, it chances unfortunately that too many of the observations which were made seem to have been intended to resolve problems already long since disposed of. Precisely as we find that the American observers in 1869 were for the most part little familiar with the history of former eclipses, so during the late eclipse the larger proportion of the observers would appear to have been quite unprepared for phenomena which have been described over and over again by the observers of past eclipses. It would seem as though that preliminary examination which the Astronomer Royal so earnestly advocated as respects modes of observation, might also have been well extended to the historical details of eclipses.

Broadly stated, the object of the expeditions was the determination of the real nature of the corona—that crown of glory which never fails to make its appearance when the moon has concealed the whole of the sun's disc. The views of astronomers and physicists respecting this striking appearance had been growing gradually clearer, one theory after another being disposed of until certain very definite issues alone remained to be considered. The theory that the glory is a lunar phenomenon had been rejected because modern observations prove so conclusively that the moon has not an atmosphere of appreciable extent. So, also, the theory that the corona is due to the diffraction of the solar rays as they pass by the moon, had been shown to be erroneous. But another theory which seemed much more worthy of support had also been successfully oppugned: the fact that all round the sun a glory of bright light appears when the sun's disc is concealed, suggests, on the face of it, that the sun has an atmosphere of great extent, either self-luminous or brilliantly illuminated by the sun himself. This view had been supported by astronomers of eminence, despite certain difficulties which it undoubtedly presents; but recent researches had caused grave doubts to be cast upon this imagined solar atmosphere. Let us briefly consider the nature of these researches.

The coloured prominences which are seen around the sun during total eclipse had been shown to be gaseous, a chief element in their constitution being the gas hydrogen. The coloured *sierra*, too, which appears at a lower level—that is, closer to the sun—had been found to be as its appearance implied, almost identical in structure. During the spectroscopical examination of these objects, peculiarities of appearance had been noted in their characteristic bright lines which seemed to throw important light upon their nature, and, indeed, on the general habitudes of the solar atmosphere. The peculiarity which now specially concerns us is *this*, that the spectrum, though undoubtedly indicating an increase of pressure with approach to the sun's surface, yet by no means indicated such an increase as was to have been expected if the sun has an atmosphere of considerable extent. The researches of Plucker, Hittorf, Frankland, and Huggins had shown that the spectrum of hydrogen varies according to the circumstances of pressure and temperature under which the gas subsists; and the widening of the hydrogen lines had come to be regarded as in a special manner indicative of increase of pressure. Mr. Lockyer to whom Frankland exhibited the spectrum of hydrogen under varying conditions, found thus a new significance in the observations made at the telescope. He found that, so far as the evidence of the laboratory experiments could be depended upon for determining pressure, the glowing hydrogen even at the base of the *sierra*[*] exists at a pressure considerably less than that of our own atmosphere at the sea-level. Wüllner assigned the limits of pressure at the sun's surface at two inches and twenty inches of the mercurial barometer (under the earth's attraction).

This seemed to dispose conclusively of the theory that the corona is a solar atmosphere properly so-called. Without accepting the somewhat *bizarre* notion put forward by Mr. Lockyer, that the *sierra* is itself the solar atmosphere, we seem yet fairly led to believe that that atmosphere into which the glowing hydrogen which forms the prominences has been flung, and in which smaller prominences and the remains of larger ones probably commingle to form the sierra, can extend but to a moderate distance above the summits of the loftiest prominences. For undoubtedly an atmosphere extending even to but 80,000 miles from the sun (and many prominences are far higher) and subjected to his enormous attractive power, could not fail to exert a pressure enormously exceeding that of our own atmosphere at the sea-level, unless this solar atmosphere were of extreme tenuity. I know not, indeed, whether even our so-called vacuum-tubes ought not to be regarded as densely filled with matter, by comparison with the outer regions of a solar atmosphere, if, with a height of some 100,000 miles at the very least, such an atmosphere is to exert at the sun's surface no greater pressure than our own.[†]

It was probably such considerations as these which led to the adoption by Mr. Lockyer of Faye's idea that the corona is a phenomenon due merely to the passage of the solar rays through our own atmosphere. It seems at a first view, more especially to those unpractised in considering the relations of tridimensional space, that when the sun is directly behind the moon, as in a total eclipse, his rays passing grazingly by the moon, must so illuminate our own atmosphere as to exhibit a glory of light—a real radiated corona—around the dark disc of the moon; and it was this consideration—plausible, but beyond question erroneous—which was the original basis of Faye's "atmospheric glare" theory. Few but mathematicians would have been prepared to hear—as Baxendell, Curtis, Harkness, and others at once pointed out—that the illumination by direct solar light could not in any ordinary total eclipse cause a ray of light to come from a space of many degrees in width all round the place of the eclipsed sun. A negative corona, that is, a corona of blackness softening off all round towards a background of considerable brightness, would be the real appearance resulting from the effect of direct sunlight.*

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[*] By *sierra* I mean the coloured layer or envelope of prominence matter to which the name of *chromosphere* has been applied. *Sierra* harmonises so well with the word *prominences*, that it is to be hoped this term, employed so many years before by Airy, Leverrier, and others, will before long dispossess so questionable a title as "the chromosphere."

[†] There are few problems more difficult than those suggested by the subject here touched upon. An atmosphere of hydrogen 80,000 miles deep, and at the upper limit existing at the very lowest pressure which our physicists can be assumed to have attained by any process of exhaustion, would yet at a depth of 60,000 miles, under the action of solar gravity, exert a pressure exceeding fully a hundredfold that of our own atmosphere at the sea-level. Nor is the difficulty removed by supposing, as I have done in my Treatise on the Sun, that the real pressure close by the photosphere exceeds manifold Mr. Lockyer's estimate. This supposition, shown by Professor Young's observations last December to be correct, yet leaves the pressure at a depth of 60,000 miles below the summits of the loftiest prominences, or at a height of twenty or thirty thousand miles above the photosphere, altogether less than can be explained by any physical relations we are familiar with at present.

This having been proved, the atmospheric glare theory was modified by the introduction of a supposed action exerted by the moon on the solar rays. The nature of this action was not definitely (or indeed at all) stated; but the theory thus modified agreed, so far as it went, with La Hire's, which had been long regarded as untenable. As a matter of fact, also, while introducing fresh difficulties, it removed none of those urged by Baxendell and others.

It is bare justice, however, to the supporters of the atmospheric glare theory to point out that, as the eclipse of last December approached, the objectionable features of the theory were removed, and views were put forward which undoubtedly agreed much better with observed facts. Let me not be misunderstood, as respects my use of the word objectionable. To an erroneous theory, in the abstract, no objections need be made; since the enunciation and demonstration of the true theory disposes of the incorrect one without occasion for a special refutation. But it sometimes happens that the enunciation of an erroneous theory at a particular time may interfere with the progress of observation—either by distracting the attention of observers, or by causing them to under-estimate the interest of the object which they are to study. And beyond question, if the corona might by any possibility be no more noble a phenomenon, no better worthy of study, than the radial beams of light which extend from between clouds over large arcs on the heavens, the enthusiasm of eclipse observers might well have been diminished. It was, indeed, on this account that, more than a year ago, I earnestly and persistently dwelt upon the objections, simple and obvious though they are, against the atmospheric theory, pure and simple, or modified so as to include lunar action. The new theory, fortunately adopted early enough to prevent the mischief I had feared—was not only far more accordant with observed facts, but even if erroneous could in no way interfere with the work of observers.

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* On this subject, Sir John Herschel writes to me—"Placing the limits of what may properly be called the earth's atmosphere at 100 miles high, then at the borders of a circular section subtending an angle of about 23 degrees in diameter every trace of light reflected in our atmosphere would be quite evanescent." This precisely accords with the objections I urged last year against the atmospheric glare theory as originally propounded.

It was now urged that the bright light seen close around the moon during total solar eclipse may be the real cause of the atmospheric glare which the theory requires in explanation of the fainter light seen farther from the moon.* This, in fact, corresponds with what I had myself pointed out in oppugning the atmospheric glare theory ("Monthly Notices of the Royal Astronomical Society" for March, 1870, p. 141), when I wrote that "the only light which can reach this part of the atmosphere" (the part towards the moon's place) "is that from the chromosphere and the coloured prominences, or from the earth and surrounding illuminated air." It is obvious that the air above and around the observer of a total eclipse must be lit up by the prominences and sierra, since these are actually seen; and, *à fortiori*, it must be lit up by that intensely bright inner portion of the corona which has so often led the observers of total eclipses to suppose that a ring of real sunlight has, in fact, remained unconcealed; though it must be noted at the same time, as I pointed out in the paper just quoted, that the light from this source "would extend over the moon's disc, since it would illuminate the air between the observer and the moon's body." This, indeed, as we shall presently see, has been abundantly confirmed by the observations made during the recent eclipse.

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* As it has been denied that the atmospheric theory had been in any way modified, I may as well remark at this point that my only means for determining what the original theory was, had been the words used in its enunciation. They asserted that "the corona is nothing else than an effect due to the passage of sunlight through our own atmosphere," and they were understood by Baxendell, Curtis, Harkness, myself, and others, to mean that the air seen towards the moon's place is lit up by direct sunlight. Nor was exception taken for several months to the fact that our objections were founded on this supposition. Last June, however, it was said that the theory had been misrepresented, and in *this* way, that no notice had been taken of "a possible lunar action" really included in the theory. Here, it should seem, was the opportunity for introducing as a part of the theory the possible chromospheric action now believed in; yet nothing was said of it. In writing on the late eclipse, however, Mr. Lockyer says of his own views, after the eclipse of 1869:—"I thought the explanation still possible which regarded the corona as of terrestrial origin; that is, which assumed it to be an appearance due to the presence of light" (no longer sunlight, it will be noticed), "in our own atmosphere. The problem was one of such difficulty that there seemed a possibility that, by some unexplained cause, some of the solar light might be diffused and beat out of its course, and then, mixing up with the light of the chromosphere, give us a sort of continuous spectrum; in other words, that as the eye perceives a bright irregular region or glare round the uneclipsed sun, an effect due to our atmosphere, so also the eye might perceive a bright irregular region or glare round the *uneclipsed chromosphere during eclipses*, due also to our atmosphere." These are, indeed, other words, for they have another meaning. In fact, if the atmospheric glare theory had originally been propounded in the vague form here assigned, it would have escaped attack. The "possibility" that "a sort of" effect might result from "some unexplained cause" would be difficult to disprove.

It is clear that, even though the corona thus accepted as real by the supporters of the glare theory, should, in fact, be but a portion of the complete corona, the enunciation of the theory could in no way interfere with the observations to be made by the eclipse expeditions. On the contrary, the very attempt to distinguish where the real corona ended and the atmospheric corona began, could scarcely fail to be rewarded by results of extreme importance.

Now, so far as the evidence available before the late eclipse was concerned, we may select direct observations formerly made, and the spectroscopic observations made during the Indian and American eclipses (of 1868 and 1869) for consideration. Photographic evidence was not wholly wanting, but so far it was certainly unsatisfactory. Polariscopic evidence, again, had seemed (and still seems) most vague and contradictory.

But direct observation had already revealed many facts bearing most importantly on the question of the corona's real extent.

To begin with, on the very point which was assigned strangely enough by the organising committee as the principal object of the expeditions, "the differentiation of the outer layers of irregular outline, and a stratum, say some 5′ or 6′ high close around the sun," the history of coronal research is decisive. Plantade and Capiés, in 1706, had already detected the difference, which 164 years later has been called "the key-note of all the observations" made last December. In 1842, Arago had dwelt specially on the same point. In fact, nothing in astronomy has been more thoroughly determined than the general uniformity of the coronal light up to a certain moderate distance from the sun, where a rapid but not sudden degradation takes place.

Yet, again, on the question whether the fainter and radiated part of the corona, even to its extremest extent, actually belongs to a solar appendage, the history of former researches speaks very plainly. These radiations have been seen to remain unchanged in position, not only during totality, but for twenty, thirty, even forty seconds afterwards. They have also presented the same figure as seen from distant stations. So that, setting aside, as we justly may, all the accounts of seeming changes in certain radiations (since such changes may so easily be explained, or are, rather, on any theory to be expected), the positive evidences, which there is no misinterpreting, force on us the conclusion that the true solar corona has radiations extending to enormous distances from the sun.

As respects the spectroscopic evidence, there seemed room for doubt. Lieutenant-Colonel Tennant had in 1868 obtained from the corona a faint continuous spectrum, without either dark lines or bright lines. The American astronomers in 1869 had seen different spectra: in some cases a single bright line on a continuous spectrum, in others a bright line and two fainter ones, while in yet others a continuous spectrum was alone discerned.*

There was little room for doubt as to the significance of these observations. In the "Quarterly Journal of Science" for October last, I pointed out reasons for believing that all the observations should be accepted, the apparent difference between the results being regarded as due in all probability to a difference in the opening of the slit of the spectroscope. This view,—abundantly verified by the observations made during the late eclipse, leaves us a double spectrum to interpret,—a continuous spectrum and a spectrum of bright lines. Assuming that the background of the spectrum really is continuous—that is, that no dark lines exist in it—we should infer that the source of light is matter in the solid or liquid form, and incandescent through intensity of heat. It is possible, however, that dark lines exist, but are unseen on account of the faintness of the spectrum; in which case we may regard this portion of the light as reflected sunlight. As respects the part of the light which gives the bright line spectrum, we should infer that the source of light is luminous gas or vapour,—with this *proviso,* that we need not necessarily have to deal with masses (still less with a complete atmosphere) of such gas, but possibly with the light due to such electric discharges as are assumed to cause the appearance of our own auroras. This view seems at least far from improbable, and it is supported by the fact that the conspicuous bright line of the coronal spectrum is one of the principal lines of the auroral spectrum. Assuming it to be true, we should remain in doubt whether the vapours corresponding to the lines exist as vapours in the space traversed by such electric discharges, or whether those lines indicate the nature of the particles or bodies between which the discharge passes.

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* I may take this opportunity of correcting a mistake which has been made—in these pages and elsewhere—with respect to Professor Young's observations of the coronal spectrum in 1869. He saw, indeed, three lines, but he was not sure that more than one of these—the line 1474 Kirchhoff—belonged to the corona. "I have felt," he writes, "somewhat annoyed by finding the other two lines put on the same footing as 1474." This is a point requiring correction, though surely not calculated to annoy. The mistake arose from the inexact wording of the American reports; and may be compared to the erroneous account given by the American observers (including Professor Young) in 1869 of the observations made by Tennant in 1868—his continuous coronal spectrum appearing in their accounts as a spectrum showing the solar dark lines.

But doubts still rested on the observations made by the American astronomers. Observers in Europe seemed disposed to question the accuracy of their American fellow-workers. "The evidence furnished by the American observers," wrote Mr. Lockyer, "is *bizarre* and puzzling to the last degree;" and he referred to observations of his own, causing him to "hesitate to regard the question as settled." The fact, also, that a double spectrum had been seen, was referred to by Mr. Lockyer as "hard to understand, unless we suppose the slit to have been wide, and the light faint, in either of which cases final conclusions can hardly be drawn either way."* With a caution not very complimentary to American men of science, their results were relegated to the eclipse of 1870, for confirmation or disproval by European observers.

Fortunately, in this respect, the observations of last December were most satisfactory and conclusive. One of the first telegrams received from the shadow-track was one in which Mr. Lockyer admitted that the observations made by the Americans in 1869 had been confirmed. And the first detailed account published in England, in the form of a letter addressed to the Editor of the "Daily News," by Father Perry, the head of one of the observing parties sent out to Spain, exhibited very decisively the same result. The following extract from Father Perry's narrative may fitly introduce the more detailed evidence to be subsequently given. Knowing that the unfavourable sky would render observations with larger spectroscopes quite impracticable,

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* From an article in the first number of "Nature," (Nov. 4, 1869.)

"I desired Captain Maclear," writes Father Perry, "to observe with a small direct-vision Browning spectroscope, attached to a 4-inch telescope." "Immediately totality commenced, the ordinary solar spectrum was replaced by a faint diffused light, and bright lines." "There were no dark lines—that is to say, none of those lines which are present in the solar spectrum. When the spectroscope was directed to a distance of about 8′ from the edge of the moon's disc, the same lines remained visible. The centre of the moon was then tried, and the bright lines were still seen, but only half as strong as before." *

But let us turn to Professor Young's account of his own observations, and those made by his fellow-workers.

First of all, and perhaps scarcely inferior in interest to anything learned during the recent eclipse, there is Professor Young's observation of the reversal of all the dark lines of the solar spectrum close by the limb at the commencement of totality. I had, at page 295 of "The Sun," ventured to express my belief that the Fraünhofer dark lines would be found to have their birth-place here (and not, as surmised by Mr. Lockyer, beneath the visible limits of the photosphere); and it was, therefore, with some pleasure that I read the following confirmation of my views on this point:—"With the slit of Professor Young's spectroscope placed tangentially," writes Professor Langley, "at the moment of obscuration, and for one or two seconds later, the field of the instrument was *filled with bright lines*. As far as could be judged during this brief interval, every non-atmospheric line of the visible spectrum showed bright; an interesting observation confirmed by Mr. Pye." "From the concurrence of these quite independent observations, we seem to be justified in assuming the probable existence of an envelope surrounding the photosphere and beneath the chromosphere, usually so called, whose thickness must be limited to two or three seconds of arc, and which gives a discontinuous spectrum consisting of all or nearly all the ordinary lines, showing them, that is to say, *bright* on a dark field." On this point Professor Young remarks, that "the sudden reversal into brightness and colour of the countless dark lines of the spectrum at the commencement of totality, and their gradual dying out, was the most exquisitely beautiful phenomenon possible to conceive, and it seems to me to have considerable theoretical importance. Secchi's *continuous spectrum* of the sun's limb" (the existence of which had been so strenuously denied) "is probably the same thing modified by atmospheric glare,—anywhere but in the clear sky of Italy so much modified, indeed, as to be wholly masked."

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* It will be noticed that this result exactly accords with my anticipations in the above quoted passage from the Monthly Notices of the Royal Astronomical Society. No stronger proof can be required of the fact that the illumination in our air is not the "sunlight" of the atmospheric glare theory, than this circumstance, that towards the moon's disc, where we have this illumination pure and simple, we get a spectrum of bright lines (instead of a faint solar spectrum).

Now, with respect to the lines seen in the spectrum of the corona, it is to be noted that Mr. Abbay, "observing at Xeres with a spectroscope of two prisms of 45° belonging to Professor Young, saw the bright lines C, D, and F, and afterwards another line rather more bright than F on the less refrangible side of B." But it is worthy of notice, as pointed out by Professor Young, that with a hazy sky as at Xeres, and, indeed, all the Spanish stations, it is impossible to feel sure from the mere appearance of a line that the part of the corona towards which the spectroscope is directed really has a spectrum showing that line. In fact, the appearance of the C line of hydrogen as far as 6′ or 7′ from the sun, "far above any possible hydrogen atmosphere," says Young, effectually demonstrates this, even if it had not happened that Mr. Abbay had seen the C and F lines on the moon itself.

Fortunately, however, Professor Young has been able to establish in a highly satisfactory, or rather demonstrative, manner the fact that light coming from a region much more than 6′ or 7′ from the sun's limb gives the bright line 1474 Kirchhoff,—the very line be it noted respecting which the gravest doubts had been expressed. Of the four spectroscopes employed by his party, "two," he writes, "were what might be called *analysing*, and two *integrating* instruments." The former gave the relative brightness of the several lines in the spectrum from a definite portion of the corona or prominences; the other showed the relative quantity of light corresponding to different bright lines when the whole of the corona supplied light to form the spectrum. Now the analysing spectroscope showed the line 1474 very much fainter than the C line of hydrogen. "Even during totality," says Professor Young, "1474 can hardly be called conspicuous in an analysing instrument, while C blazes like a red Sirius;" and he concludes that C is at least 25 times and perhaps 50 times as bright as 1474. Now in the integrating spectroscope the order of brightness is reversed. The line 1474 is appreciably brighter than the line C, insomuch that Professor Young assigns as the numbers indicating the relative brightness of these lines, 100 for the line 1474 and 72 for the line C. Only one explanation is available, namely, that the area of the region supplying light whose spectrum shows the 1474 line must exceed the area of the region whose light shows the C line, in the proportion which 100 × (from 25 to 50) bears to 72, or from 35 to 70 times. Professor Young, assigning to the coloured sierra and prominences an apparent area equivalent to a ring a quarter of a minute high round the sun, concludes that the self-luminious corona would be "equivalent to another ring from 8′ to 14′ high."* The irregular nature of the inner part of the corona would also seem to leave no escape from the conclusion that this portion cannot suffice to give all the light showing the 1474 line, unless we suppose the inner corona to extend in places considerably more than 8′ from the sun. Professor Young, therefore, concludes that "we have surrounding the sun, beyond any farther reasonable doubt, a mass of self-luminous gaseous matter, whose spectrum is characterised by the green 1474 line. The precise extent of this it is hardly possible to consider as determined, but it must be many times the thickness of the red hydrogen portion of the chromosphere; perhaps on an average 8′ or 10′ with occasional horns of twice that height. It is not at all unlikely that it may even turn out to have *no upper limit*, but to extend from the sun indefinitely into space."†

As respects other spectroscopic evidence, little need be said. None of the other observers save Lieutenant Brown obtained results which differ essentially from those already recorded. Lieutenant Brown was unable to detect the 1474 line, but as he used a single-prism spectroscope, whereas the invisibility of the line depends on a certain amount of dispersion being obtained, his failure can be readily interpreted. The Italian observers at Agosta saw two very brilliant bands. "Father Denza," writes Secchi, "executed the part assigned him to admiration. He saw the protuberances clearly, and choosing the most brilliant part of the corona, he directed the spectroscope towards it; and whilst the assistant kept it fixed, he was able to distinguish plainly the spectrum of the protuberances from that of the corona. This last was continuous, and had two very brilliant bands, one in the green near Fraünhofer's E, and another in the yellow-green. . . . . The shortness of the time, and the difficulty of calculating the scale would not allow us to fix the lines in question with more precision."

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*In "Nature" this is written 8′ to 16′ high," but this is obviously due to a mistake; and Professor Young probably wrote the passage as I have put it. A ring 16′ high round the sun's disc would exceed a ring one quarter of a minute high, about 80 times in extent, instead of about 70 times.

†In one of the most humorous paragraphs I ever remember to have read in a scientific paper, Professor Young assures the advocates of the atmospheric glare theory that "in the main" he concurs with their views "*with the exception* that he is disposed to" negative the only remaining special point which they had not conceded. The corona had been gradually admitted to be solar as respects a greater and greater extension from the sun; but for all above 6′ or 7′ from the sun, and specially for the radiations, a contest was still maintained with the object of proving these parts to be terrestrial. Professor Young placidly hands over to the sun these last remaining portions of the corona, assuring the advocates of the terrestrial theory that *in all other respects* he agrees with them.

As regards the agreement of the line which has hitherto been spoken of as Kirchhoff's 1474, with that well-known iron line, Professor Young remarks, "the difference, if any (and I have not found the slightest reason to suspect the least want of coincidence in observations with the whole dispersive power of thirteen prisms) is less than 1-10th of one division of Kirchhoff's scale. There is no solar dark line but 1474, therefore, with which this bright line can be supposed to agree.

There seems to be no valid reason for regarding this line as belonging to a new element, as has been suggested. In fact, without denying the possibility that elements unknown on earth may exist in the interplanetary spaces, or elsewhere, it could only be on the score of very striking evidence that terrestrial physicists would admit the actual existence of such elements in any special instance. In this case, we have a line which appears to be absolutely identical with a known iron line, and is also recognisable in the spectrum of the terrestrial aurora. We may reasonably inquire, therefore, under what conditions the metal iron can supply this line without exhibiting the other lines belonging to the iron spectrum; but we certainly seem to have no evidence favouring the supposition that a new element is in question. On the contrary, the evidence, as far as it goes, is opposed to such a supposition.*

But we must now turn to the evidence afforded by direct observation, supplemented as such observation has in this case been by the evidence of photographic records.

I lay no stress on the recognition of a very striking difference between the luminosity of the inner part of the corona and that of the outlying and seemingly radiated portion. For this difference is a phenomenon which has been known for more than a century and a half, and only that forgetfulness of past labours and their results which has so constantly characterised eclipse observation, could have caused any stress to be laid on the recognition of so well-worn a fact.

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* We now have Zöllner's study of the auroral spectrum to show that in all probability the extreme tenuity of the medium, through which the electrical discharges causing the auroral light take place, is to be regarded as the secret of the nature of the auroral spectrum. The presence of one line of iron both in the auroral and coronal spectra, while all the remaining lines are absent, may thus come one day to be satisfactorily interpreted.

For a similar reason, I should be disposed to lay little stress on the evidence obtained respecting the coronal radiations, conceiving as I do that all that could be learned respecting their general characteristics had been long since ascertained. But as to many, unaware of the immense mass of evidence already available, this feature of the corona has seemed worthy of special study, it will be well to consider what fresh evidence has been obtained during the recent eclipse. Fortunately, I am able to do this without altogether repeating an oft-told tale,—partly because photography has supplied a new kind of evidence on the point, and partly because a new fact has been brought out in connection with it.

Considering the unfavourable circumstances under which the recent eclipse was viewed, and that the eclipse itself was much less important than many during which the corona has been studied, it may be regarded as fortunate that the corona presented one feature which could scarcely escape the notice of any moderately attentive observer. Opposite the south-eastern quadrant of the moon's limb, there was a well-marked gap—V-shaped—whose apex reached close up towards the moon. So close, indeed, that, as depicted in figure 9, the distance of the apex from the sun was actually less than that of the highest parts of the inner corona. *Under this gap the inner corona sinks lowest, while on each side it rises quickly up to about its mean level.* Still referring to Lieutenant Brown's picture, let it be noticed that the V-shaped gap is the most striking feature of the corona. It is the sort of feature which might well be expected to afford a crucial test of the vexed question whether the radiations really belong to some solar appendage or are merely phenomena of our own atmosphere. If it shall appear that this V-shaped gap was seen at other stations, and more particularly at stations far removed from Lieutenant Brown's, then all further question on this point becomes impossible.* It happens fortunately that, notwithstanding the unfavourable weather generally experienced, the evidence on this point is unmistakable.

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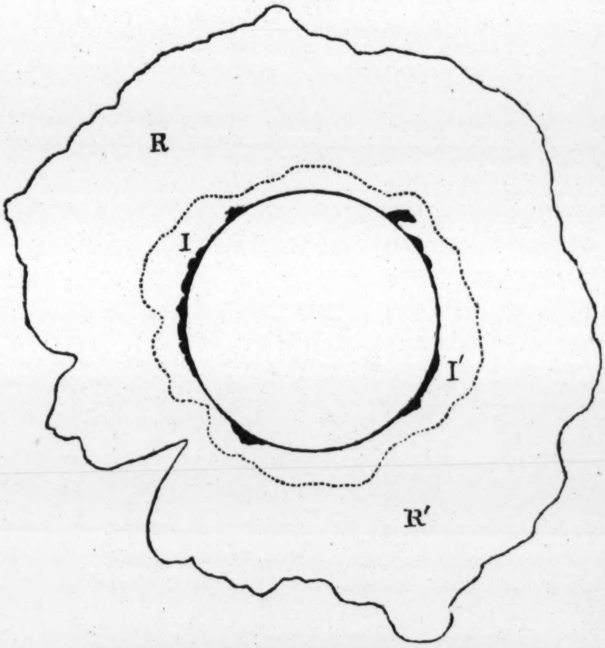
* Note, however, in passing, that if the depression of the brighter part of the corona beneath the great gap be admitted, on the strength of Lieutenant Brown's drawing, then the matter is disposed of at once; for the connection between the matter in which the gap exists and the inner corona is at once established. It need hardly be said that the shape of the inner corona could not possibly be reflected—as it were—in the illumination of our own atmosphere.

In the first place, we have the evidence respecting the corona as seen at other Spanish stations. I extract the following account from a paper in the "English Mechanic," for January 27th, 1871, which, though anonymous, shows obvious signs of having been written by a practised mathematician and a well-informed physicist. I note, also, that the account accords exactly with statements addressed to the January meeting of the Royal Astronomical Society, and with accounts I have received from different members of the expedition:—"The *corona proper*, or *glory*, or *radiated corona*—as it is variously called, extended a distance of almost the moon's diameter from the moon's limb, but not equally in every direction. It had a greater extension in four directions, at the extremities of two diameters at right angles, so as to give it the shape, roughly speaking, of a square with rounded corners. It was broken in parts, and notably by one decided V-shaped gap. This was observed not only by one party, but at three stations, San Antonio, Xeres, and La Maria Louisa, which form a triangle each of whose sides is five or six miles in length." Mr. Hudson,

FIG. 9.



The Corona as drawn by Lieut. Brown.

R, R′ the outer radiated corona.
I, I′ the inner and brighter corona.

Fellow of St. John's College, Cambridge, referring (at the January meeting of the Royal Astronomical Society) to the V-shaped gap exhibited in Lieutenant Brown's drawing (of which figure 9 is a reduced outline-view) remarked that the gap appeared to him somewhat larger; and Lieutenant Brown agreed that his drawing did not quite adequately represent the dimensions of the gap.

So far, then, the evidence is most satisfactory. It may be indeed regarded as already sufficient; since no peculiarity of our own atmosphere could possibly explain the similarity of the views seen at stations so far apart; while the fixity of the gap, a fixity respecting which all accounts agree, disposes of the theory (untenable also on other accounts), that the bounding radiations which formed the gap were due to matter near the moon illuminated by the solar rays.

But Lord Lindsay, and Mr. Willard (a member of the American observing party), had photographed the eclipse at stations near Xeres; and here was a further means of testing the matter. If the photographs showed the V-shaped gap, the evidence would indeed be irresistible.

Now, as respects Lord Lindsay's photographs, I am able to say but little. He had with great spirit undertaken his share of the observations at a time when it seemed likely that Government aid would be refused altogether. He had furthermore taken out at his own expense a fine 12¼-inch reflector by Browning. The ingenious contrivances by which the production of the successive pictures was mechanically provided for, enabled Lord Lindsay to take no less than nine photographs. But those I have seen suggest the belief that, so far as the corona is concerned, the results would have been more satisfactory had fewer pictures been taken, the exposure of each being correspondingly increased. The very clearness with which, in a most beautiful photograph of the series, now lying before me, the prominences are shown, leads me to believe that the exposure was altogether too short. We have evidence, both in the American photograph and in that by Mr. Brothers, that the actinic energy of the prominence light is much greater than that of the corona, "the light of which," says Mr. Brothers, in a letter to me, "rather resembles moonlight in this respect." A photograph, therefore, which shows the corona well cannot possibly show the prominences clearly—though the *effects* of the prominences may be very marked indeed; and, *vice versâ*, a picture in which the prominences are well-defined cannot be expected to show the details of the corona.

The American photograph is shown in the second figure of the illustrative plate. It will be seen that the extent of the field is limited, as if by a circular stop, to a space of about 12′ wide all round the moon's body. Yet the V-shaped gap is shown very distinctly indeed. It is to be noted, also, that there are obvious traces of two other gaps, one about 40° round towards the left, and the other about 70° round towards the right. Yet, again, it is to be noted that the depression of the inner corona beneath the great V-shaped gap is very distinctly indicated; and in other respects the agreement of the part of the corona shown with the indications in figure 9 is most remarkable.

But now another circumstance invites our attention. The American photograph shows the position of the apex of the gap to have been exactly midway between two prominences. The prominences are, indeed, not actually seen; but the notching of the moon's disc (caused by the eating away due to their actinic energy) leaves no question as to their existence there. I find two prominences indicated also in this position, not only in Lord Lindsay's photographs, but in some excellent drawings of the eclipse by Lieutenant Watkin, R.A., Lieutenant Sidney G. Smith, R.N., and Mr. Abbatt, F.R.A.S. Now here we have the means of ascertaining the real existence of this gap as a true solar phenomenon beyond all possibility of question,—if only another photograph taken at a distant station shall show the gap similarly placed.

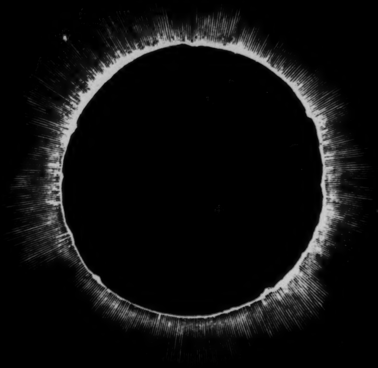
This brings me to what I take to be the crowning achievement of the eclipse expeditions—an achievement well worth all the labour and cost which those expeditions may have involved to the American, British, and Continental Governments and students of science. I refer to the admirable photograph of the corona by Mr. Brothers, F.R.A.S., taken at Syracuse, during only the last few seconds of totality—for clouds rendered the first exposures relatively ineffective. Those eight seconds, however, during which this photograph, the fifth of the series, was exposed, sufficed to give a picture whose scientific value is only surpassed by the interest which attaches to the view as the first really successful photographic delineation of the solar corona.

This photograph, of which the first view in the illustrative plate is a copy,* is well worth careful study.

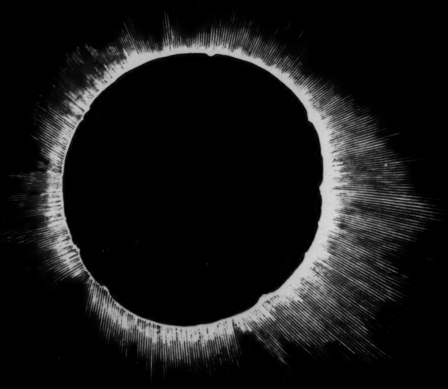
* Mr. Brothers writes to me that there are strange details in the negative which he cannot reproduce, either in photographic copies or by draughtsmanship. Their extreme delicacy defeats all attempts to reproduce them. Even the extensions seen in the plate have had to be copied directly from the negative, for they are too delicate to show in ordinary photographic copies.

THE ECLIPSE OF LAST DECEMBER

II



I



I. Mr BROTHERS' PHOTOGRAPH, TAKEN AT SYRACUSE.

II. Mr WILLARD'S PHOTOGRAPH TAKEN NEAR XERES.

In the first place, the situation of the great V-shaped gap is clearly defined, and unmistakably agrees with what is seen in the American photograph. *There* are the notches showing the places of the two prominences equally distant from the apex of the gap. And if any doubt could exist as to these two notches—or rather the prominences they indicate—being the same as those in the American photograph, that doubt would be removed by the agreement of the other prominence-notches—all round the moon's disc.

Again, the two other gaps or rifts, which are but faintly indicated in the American photograph, are plainly shown in Mr. Brothers's. We see also *why* they are less clearly seen in the former. They do not extend so far in towards the sun's globe, and are banked up, as it were, all round by masses of the bright inner corona. Their agreement in position with the indications of the American photograph, as also with what can be recognised of these delicate phenomena in Lieutenant Brown's drawing, cannot be questioned.

Let it further be noted that the agreement of the features of the inner corona as shown in the two photographs is even greater than was to have been anticipated when the undoubted effect of atmospheric illumination, in blurring and even in part modifying the details, is carefully attended to.* Here again, too, the accuracy of Lieutenant Brown's drawing is *surprisingly* confirmed. I use the italicised word advisedly, for it has very seldom happened that the corona has been satisfactorily delineated even when practised draughtsmen have attempted the work.

But two features remain to be specially noticed.

In the first place, the disc of the moon is not perfectly round but obviously compressed from east to west.† If the moon in the American view were similarly distorted, we might be led to ascribe this to some peculiarity arising from the moon's motion from west to east across the sun's face. But as the American drawing shows only a glare of light on the moon's eastern and western limbs, this explanation

* Mr. Brothers having kindly sent me copies both of his own photograph and the American one (the former enlarged to the scale of the latter) I had hoped to provide chalk drawings for the engraver, and so to have preserved the originals for my own use. But I was foiled in this, by the fact that I found myself quite unable to reproduce the close resemblance which exists between the two photographs. Even when I made both drawings from one photograph, the resulting views were not so similar as the two originals. As I write, I have not seen the proofs; but I fear that no engraver, however skilful, can make the two pictures so like as to give a sufficient idea of the evidence they really afford.

† The drawings are placed about as the sun actually appeared from the Syracusan station; so that east and west correspond respectively to the left and right sides of the drawings.

is not available. Fortunately we *have* an explanation, and, carefully considered, it throws some light on an interesting question of solar physics. Mr. Brothers tells us that there was a good deal of wind during totality. Now the image of moon, prominences, and corona being more swayed in a horizontal than in a vertical direction, we can understand that the moon's horizontal diameter would be more affected than the vertical diameter. The actinic energy of the prominences far exceeds that of the corona, so that the prominences would leave their track (as it were) unmistakably impressive on the eastern and western limbs of the moon. But it needs only a brief study of the picture to see that the western limb has suffered much more than the eastern. At first sight this seems perplexing, especially when we note that the *whole* of this limb has suffered, not such parts only as were opposite lofty prominences. But when we consider that the totality was drawing to a close at the time, we see that this is precisely what should be expected; for the sierra would have come into view on the western side, while the *sierra* on the eastern side would be concealed by the advancing moon. It was the image of the *sierra*, then—swayed backwards and forwards by the wind across the western limb of the moon—which caused the remarkable flattening of the moon on that side.[*]

The second feature is the great extent both of the inner and outer corona on the western side. Now, as respects the inner corona, a portion of the excess of width on the western side must, without doubt, be ascribed to the flattening of the western side of the moon's disc. We have only to conceive the full dimensions of the moon indicated on that side to see that the inner corona there will be greatly reduced. Since, also, the moon was as far as possible towards the east—for it is to be remembered that $3^{s.}$ after the exposure was completed totality ended—we seem to have a sufficient explanation of the eccentricity of the ring-formed corona. As regards the great extension of the outer corona towards the west, it is necessary to note that the outer corona even in the negative is of extreme delicacy,—very much more delicate, in fact, than any tint which the most skilful engraver could produce; and a very slight additional atmospheric illumination would suffice to add to the apparent extension of the corona. Now we have only to conceive the actual nature of the illumination of our atmosphere during totality by the bright inner corona to see that towards the end of totality the light thus arising would be much stronger on the west than on the east,—*so* much stronger, we may readily believe, as to veil in some sort the features of the actual radiated corona on that side, and seemingly also to increase its extension.

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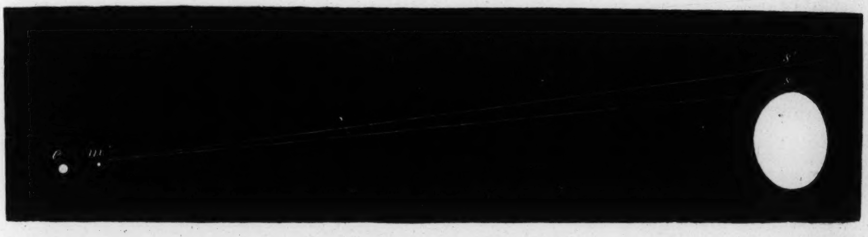
[*] We have corroboratory evidence on this point in Father Secchi's account of the eclipse. For he tells us that towards the close of the totality he was able to see the protuberances (at first there had been clouds); and he remarks that the protuberances formed a beautiful small semicircular corona. "It was full of the well-known rosy jets, of admirable shape and beauty," confirming the view that "they extend all round the sun, differing in height in different places." It will be seen that Father Secchi considers the evidence altogether unfavourable to the existence of a real envelope such as that conceived in the theory embodied in the title *chromosphere* (for the *sierra.*)

This, indeed, seems the only available explanation. For though Dr. Oudemann has suggested that matter between the earth and moon may explain the peculiarity—which was first observed in the eclipse of 1715, and has since been recognised in nearly all total eclipses—there are overwhelming objections against his theory. Those I am about to urge do not, indeed, apply to the theory as understood by Professor Young, who in the paper from which I have already quoted, remarks that a light cloud of cosmical dust, one or two hundred thousand miles above the earth's surface, and of great thickness, would account for observed peculiarities otherwise not easily interpreted. This is just, but nothing could account for such a cloud as a regular attendant of the earth and always lying towards the moon's place in total eclipses. Dr. Oudemann's theory is very different, and is also in itself far more plausible. For he supposes the cosmical dust to occupy the whole space between the earth and the sun,—to form, in fact, such a lenticular disc around the sun as astronomers have long been familiar with in theories explaining the zodiacal light. Not only is there no improbability in the general supposition, but, on the contrary, it may be regarded as absolutely certain that space around the sun, as far as our earth and farther, is in reality so occupied.

But to Oudemann's theory, propounded by way of explaining the peculiarities of the corona, there is an overwhelming objection. If we suppose that *e* (fig. 10), is the earth, *m* the moon, and *s* the sun, (the dimensions of these bodies, and the distance *e m* being enormously exaggerated compared with the distance *m s*), then whatever light might be supplied during total eclipse from a space *e m* full of the supposed cosmical dust, would be altogether surpassed by the light coming from the prolongation of the space *e m* towards *s*, up to, and *past s*. And *this* not only because the latter regions of space are so much more extended, but because the matter occupying a large portion of them must be so much more brilliantly illuminated. The obliteration of the stars by the light of the sun cannot be more complete than the obliteration of the light from the region *e m* by that from the region beyond *m*, up to, and past *s*.

The polariscopic observations, as I have mentioned, are perplexing, and in some respects seem contradictory. They seem, however, to establish these results, that (1) a considerable proportion of the coronal light is reflected; (2) that *all* the reflection has not taken place in our own atmosphere; and (3) that it is not such as would result if the sun's light were reflected from mere amorphous dust, but rather as though the reflection took place from surfaces of a crystalline nature.*

FIG. 10.



Illustrating the main objection to Oudemann's Theory of the Coronal Radiation.

And now to sum up—very briefly, for already I have exceeded my allotted space—how does our knowledge respecting the corona now stand, and in what respects has it been increased? It will be gathered from what I have already said that I attach no great weight to certain results which have elsewhere been dwelt on as the most important fruits of the expedition,—for this reason, simply that those results had been obtained long before the expeditions set forth, and were well known to all familiar with the history of physical astronomy. I take it that the fruits of the expeditions may be thus summarised :—In the first place the corona has at length been photographed, so that its peculiarities may be studied at our leisure without fear of mistakes arising from inexact delineation. Secondly, the connection between the ring-formed and the radiated corona has been demonstrated by the photographic and other evidence showing how the height of the bright inner corona corresponds with that of the outer corona. (This is a most important discovery.) Thirdly, the fact that one of the lines of the corona spectrum is identical with Kirchhoff's 1474, a line seen in the spectrum of our own aurora, has been abundantly demonstrated. Fourthly, the region in which the Fraünhofer lines have their origin has been ascertained and shown to be an atmospheric envelope (which may be some two or three hundred miles deep) lying immediately above the photosphere. Fifthly, the theory that the *sierra* is of the nature of an atmosphere has been invalidated, and the earlier opinion (which Professor Respighi had supported on the evidence of his spectroscopic observations) has been confirmed if not demonstrated,—the view, viz., that the sierra consists of multitudes of the rosy prominences resembling the large ones in all other attributes except size.

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* The account of the eclipse would be incomplete without a reference to the remarkable observations made by Sig. Diamilla Muller of the variations in terrestrial magnetism during the eclipse. He says in the "Gazzetta Ufficiale del Regno d'Italia," January 17:—"On the 22nd December the needle followed its usual course till the commencement of the eclipse. At that moment it ought to have continued its usual motion from east to west. Instead of this, soon after the first contact, its westerly motion stopped, and it retraced its steps till it reached its minimum declination at 1 hr. 58 min. (Terranova mean time) exactly at the instant of totality. From the moment of totality to that of last contact, as the disc of the sun gradually reappeared, the ascending motion towards the west began anew; and at the end of the eclipse the declination needle had returned to the precise position which it had quitted at the beginning of the phenomenon." This observation merits the most careful study; but I have not here space to indicate the conclusions to which it would seem to point.

I shall not venture to theorise in this place respecting the real nature of the corona; for the present we may remain satisfied by having learned beyond all question its real position in space, and something of its physical constitution. There are problems of extreme interest concerning it which still await solution. There are difficulties, also, which must by no means be overlooked if we would explain its phenomena aright. To future observation and research these problems and difficulties may not unsafely be relegated.

NOTICES OF BOOKS.

The Descent of Man, and Selection in Relation to Sex. By CHARLES DARWIN, M.A., F.R.S., &c. In 2 vols.; with Illustrations. London: J. Murray. 1871.

THE first impression with many on opening this work,—the announcement of which has for the last year and a half stimulated the curiosity and excited the most intense interest of the whole scientific world,—will possibly be one of disappointment, that so small a portion of it is devoted to the main subject, an attempt to trace the descent of man from those lower forms of animal life which present the closest relationship to him in structure and in mental development. The book is divided into two parts, of which the first, "On the Descent of Man," occupies only rather more than half the first volume; the second part, "On Sexual Selection," comprising fourteen out of the twenty-one chapters of which the work consists; eleven of these fourteen chapters treating of sexual selection in relation to the lower animals, the last three again applying the principles which have been arrived at to the case of man. No one, however, will read beyond the first chapter without recognising many of those qualities which secured for the author's work "On the Origin of Species" a reception almost without a parallel, and enabled it to effect a revolution all but unexampled on the current of thought in the scientific world. The lapse of twelve years has abated none of Mr. Darwin's industry in collecting a prodigious array of facts from all conceivable sources in support of every proposition which he brings forward; nor has it deprived us of that wonderful combination of humility, confidence, and appreciation of the labours of others, which makes him ever ready even to magnify the importance of facts which appear to tell against his own peculiar views, and never backward to acknowledge when he himself sees reason to change some previously expressed opinion.

Commencing with a detailed account of the homologous structures in man and the lower animals, especially in their rudimentary organs and embryonic development; and with a comparison between the mental powers of the human race and those of the remainder of the animal kingdom, Mr. Darwin then proceeds to discuss the questions of the manner in which man has been developed from some lower form, and of the particular form which has established the best claim to be considered in the light of our remote ancestor. The question naturally arises in the outset,—granted the hypothesis that man has sprung by a process of gradual evolution from some lower form, and does not owe his origin to a separate act of creation,—are we to attempt to trace this descent in one or more lines? In other words, are all the various races of mankind to be included in one species or in several? It must be borne in mind that this question is not the same as whether mankind are sprung from one original pair. When the evolutionist supposes that one species of animal or plant has been evolved out of a pre-existing closely allied form, it is not necessary to assume that all the existing individuals are descended from some one aberrant off-shoot of the original stock; the whole race may have gradually changed by the operation of the law of the Survival of the Fittest, owing to some alteration in its external environment, so that it may be impossible to draw a line of demarcation between the earlier and the later form. This is doubtless the manner in which all evolutionists must hold that man gradually arose by continuous modifications from his nearest structural relatives, the anthropoid apes; but whether in one or several lines of descent is an open question. Mr. Wallace, in his essay on "The Development of Human Races under the Law of Natural Selection," while stating the arguments on both sides of the question, sums up on the whole in favour of the primitive diversity of man; Mr. Darwin we understand to hold a different opinion. We could have wished that his views on this point had been more explicitly stated; we draw our conclusion rather from his mode of expressing himself than from any definite statement, and from the absence of any allusion to more than one such line of descent. Let us examine his arguments a little more in detail.

Starting with the main principle of the theory of Natural Selection, that all organic species tend spontaneously to vary from the parent form, generally to a very small extent, and that those variations only survive and become hereditarily perpetuated which present some advantageous point of structure in comparison with their fellows, Mr. Darwin applies this principle to the case of man, stating that, "in order that an ape-like creature should have been transformed into man, it is necessary that this early form, as well as many successive links, should all have varied in mind and body. It is impossible to obtain direct evidence on this head; but if it can be shown that man now varies—that his variations are induced by the same general causes, and obey the same general laws, as in the case of the lower animals—there can be little doubt that the preceding intermediate links varied in a like manner. The variations at each successive stage of descent, must, also, have been in some manner accumulated and fixed." One of the earliest changes must have been in the shape of the hands and feet. The hands and feet of the anthropoid apes are admirably adapted for climbing trees and obtaining their food. Baboons, however, which frequent hilly and rocky districts, and only from necessity climb up high trees, habitually use their feet for walking along the ground, and have acquired almost the gait of a dog. In order to enable man to obtain mastery over those arts which have raised him so far above even the most intelligent of his nearest relatives, it was necessary that his hands should not be used for the purpose of locomotion, and hence that he should acquire the habit of walking erect on his hind legs. This great change having become effected in the course of countless generations, the rest followed more easily, and probably more rapidly. "The pelvis would have to be made broader, the spine peculiarly curved, and the head fixed in an altered position, and all these changes have been attained by man. Professor Schaafhausen maintains that 'the powerful mastoid processes of the human skull are the result of his erect position;' and these processes are absent in the orang, chimpanzee, &c., and are smaller in the gorilla than in man. Various other structures might here have been specified which appear connected with man's erect position. It is very difficult to decide how far all these correlated modifications are the result of natural selection, and how far of the inherited effects of the increased use of certain parts, or of the action of one part on another. No doubt these means of change act and react on each other; thus, when certain muscles, and the crests of bone to which they are attached, become enlarged by habitual use, this shows that certain actions are habitually performed, and must be serviceable. Hence the individuals which performed them best would tend to survive in greater numbers." The increased use of the hands and hand-made weapons, and the consequent decreased use of the jaws and teeth in fighting, would tend at once by disuse to a reduction of the great development of the jaws, and especially of the canine teeth of the males, which is the most conspicuous element in the facial difference between the higher apes and man. The absence of hair on the back and other parts of the body, which presents so great a difficulty to Mr. Wallace, arose probably from that sexual selection which Mr. Darwin enters into so largely in the latter part of the book, originating in the female from a dawning sense of beauty in the male, and hence becoming transmitted by inheritance to both sexes. With the increased use of the hands and gradual discovery of the arts, the social instincts were developed, man became more and more dependent on his fellow, the brain was brought more and more in requisition, and hence, together with the skull, increased in size; the intellect became increasingly developed, and hence half-savage man gradually emerged into definite existence.

To return to the question of the single or plural origin of man; there is much to be said in favour of the former idea. The fertile interbreeding of all races with one another; the discovery of the use of fire and of other arts in pre-historic times, and many other circumstances, point to this conclusion. On the other hand, there are many grave difficulties in the way, if we look to natural or sexual selection as the only means by which man has raised himself above the level of the brutes. Our nearest living relatives, the anthropoid apes, belonging to the section *Simiadæ*, include the orang, the gibbons, the chimpanzee, and the gorilla. Of these the gibbons, or *Hylobates*, comprise half-a-dozen species scattered over the Islands of Java, Sumatra, and Borneo, the Malayan Peninsula, and a portion of the Continent of Hindostan; the true *Simia*, or orang, is found only in Sumatra and Borneo; while the *Troglodytes*, or chimpanzee and gorilla, belong to North Africa. Fossil remains of other anthropoid apes have also been found in South Europe; so that we may consider the family to have extended at one time over the whole of the warmer portions of the Old World. Now, seeing that the raw material, so to speak, of the human race had this wide distribution, it is difficult to explain the fact,—if we suppose with Darwin that no internal predisposing cause has been at work,—that in one spot only in this vast region have the circumstances been sufficiently favourable to evolve from the pre-existing materials the more highly developed form. The ordinary course of nature would have been for one race of men to have become developed in the Islands of the Indian Archipelago, another in Africa, and another possibly in South Europe, so distinct that they could not be confounded with one another, and each adapted to the circumstances in which he was placed. We may take a similar instance from the equine tribe, which, until the discovery of the fossil *Hipparion*, was considered to be a family without near relatives in past or present times. The equine progenitor, however, the *Hipparion*, has become developed into the horse, the ass, the zebra, and some other forms, forming species so absolutely distinct that they either refuse to interbreed or produce only sterile hybrids.

Man presents a very singular exception to the general law, that widely distributed species belong to genera which include a large number of species; in other words, have many very near relatives. A case of extreme differentiation, similar in some respects to that of man, is furnished by the giraffe; but Mr. Mivart has shown in his "Genesis of Species" the difficulties in the way of the theory that the giraffe has been developed from other African genera of *Ungulata* by the operation of natural selection alone.

Again, the gap between the higher apes and man is, by the admission of all who have studied the subject, so enormous, that we might fairly expect that geological researches would have laid bare some of the intermediate links. Remains of anthropoid apes have been discovered in Greece, but they are manifestly those of anthropoid apes and nothing else. Remains of man have been found of enormous antiquity, contemporary with the mammoth and the woolly rhinoceros, but we have the authority of Professor Huxley (who has probably given the subject more attention than anyone else, and who is assuredly not biassed against the developmental hypothesis) for asserting that the Engis and the Neanderthal skulls "can in no sense be regarded as the remains of a human being intermediate between man and the apes," and that "they do not seem to take us appreciably nearer to the lower pithecoid form." Nor does the ordinary reply of the imperfection of the geological record seem to us to apply here. The remains of the animal or animals which formed the link between man and the apes would be preserved in the most recent formations, nearest the surface, where they would have been subjected to the least destructive influences; and it is strange that no trace of them has yet rewarded the labours of the many diligent searchers in this field.

It may be some comfort to sensitive persons to hear that we need not look in the Zoological Gardens or elsewhere for any one species of ape to which we are bound to offer the homage of paternity, that the gorilla, chimpanzee, orang, and gibbons are, after all, nothing more to us than very remote cousins of the same generation, but deprived of the same advantages of circumstances or of education. Mr. Darwin believes that "man is descended from a hairy quadruped, furnished with a tail and pointed ears, probably arboreal in its habits, and an inhabitant of the Old World;" though again we are not specifically informed whether this creature is the missing link between some extinct anthropoid ape and ourselves, or the common ancestor of the whole of the *Simiadæ*. If the former, how do we arrive at the development of the tail? the suppression of which Geoffroy St. Hilaire believed to be indispensable to the enlargement of the opposite extremity of the spinal cord.

The subject of Sexual Selection is treated at great length, and with a most instructive wealth of illustration, in the volumes before us. In the lower divisions of the animal kingdom sexual selection appears to have done little or nothing; it commences its operation apparently with the lowest classes of the *Arthropoda* and *Vertebrata*, and its development runs to some extent parallel with that of the intellectual faculties. "In the most distinct classes of the animal kingdom, with mammals, birds, reptiles, fishes, insects, and even crustaceans, the differences between the sexes follow almost exactly the same rules. The males are almost always the wooers; and they alone are armed with special weapons for fighting with their rivals. They are generally larger and stronger than the females, and are endowed with the requisite qualities of courage and pugnacity. They are provided either exclusively or in a much higher degree than the females, with organs for producing vocal or instrumental music, and with odoriferous glands. They are ornamented with infinitely diversified appendages, and with the most brilliant or conspicuous colours, often arranged in elegant patterns, whilst the females are left unadorned. This surprising uniformity in the laws regulating the differences between the sexes in so many and such widely separated classes, is intelligible if we admit the action throughout all the higher divisions of the animal kingdom of one common cause, viz., sexual selection." Some of the most interesting chapters in the whole book are those in which Mr.

Darwin details the varied contrivances which are found in different sections of the animal kingdom, by which the male is enabled to please or to charm the female by superiority in colour, in adornment, in form, or even in voice; and shows that the female does exercise a power or choice in selecting the male which pleases her best. He follows Montague and Bechstein in affirming that "the males of song-birds and of many others do not in general search for the female, but, on the contrary, their business in the spring is to perch on some conspicuous spot, breathing out their full and amorous notes, which, by instinct, the female knows, and repairs to the spot to choose her mate;' and that "the female canary always chooses the best singer, and that in a state of nature the female finch selects that male out of a hundred whose notes please her most." With an admirable method and logical sequence, Mr. Darwin traces his phenomenon through the animal kingdom, and points out the effect it must have had in gradually improving the race by giving the more fortunately endowed males a preference as the parents of the next generation. Many of the peculiarities of the human species are traced to the same cause, and especially the gradual diminution in the amount of hair in both sexes, through the development of the sense of beauty.

But, granting the establishment of this principle, what do we gain by it? It seems to us, indeed, to throw the difficulty of accounting for the origin of the higher forms of life only one step backward. The best favoured males are selected in preference by the females; but whence comes the power of the female to discriminate between her rival wooers? It is obvious that for a hen canary to distinguish between the song of one bird and another, which even to our ears present only a slight shade of difference, or for the turkey hen to pick out her partner who struts in the most fascinating style or displays the most gorgeous plumage, requires the assumption of the possession on her part, not only of powers of observation of a very high order, but also of a not contemptible æsthetic principle, which must gradually have been produced by insensible accumulations, and cannot have been, according to Darwinian principles, an innate gift or power. Do we, then, arrive any nearer to a solution of the principle which lies at the base of a continuous organic improvement of the race, when we carry back our position from a gradual advance in external characters in the male to a gradual advance in the female of a mental power of appreciating these external characters? We think not. While sexual selection appears abundantly sufficient to account for the one, to what cognate principle can the followers of Mr. Darwin point to explain the other? Is not the mental development of the female, in fact, a harder problem to solve than the physical development of the male?

Darwin's "Descent of Man" is a work that will long hold a place in our literature as a monument of patient and laborious research, and of great impartiality and candour in the results adduced. As the basis from which other inquirers will start their labours, it will always be invaluable. The line of argument pursued is throughout rigid and consistent; and if the conclusions arrived at are ever shown to be erroneous, it will be not so much by discovering any flaw in Mr. Darwin's line of argument, as by a substitution of other premisses for those on which he founds his hypothesis; in other words, by the discovery of some organic law or laws governing the evolution of organic forms, of which we are at present ignorant.

Whether the dogma that Natural and Sexual Selection are of themselves sufficient to account for the evolution of the lower forms of animal life, and of man from those orders which are most nearly allied to him, the progress of future research only will show.

General Outline of the Organisation of the Animal Kingdom, and Manual of Comparative Anatomy. By Thomas Rymer Jones, F.R.S.; 4th Edition; Illustrated by 571 Engravings. London: Van Voorst, 1871.

A *Manual of Zoology for the Use of Students, with a General Introduction on the Principles of Zoology.* By Henry Alleyne Nicholson, M.D., D.Sc., &c. Edinburgh and London: Blackwood and Sons, 1870.

Of the two books which we have here bracketed together, the first is a new edition of a well-known and deservedly popular text-book of Comparative Anatomy. Since the first edition of the work was published, great advances have been made in the study of zoology; the improvements in the construction of microscopes has much advanced our knowledge of the *Infusoria*; the researches of Van Beneden and Siebold have opened new fields in the embryogeny of the *Tæniadæ*; while the discovery by Steenstrup of the alternation of generation of the *Hydrozoa* has thrown a new light on many organic problems. More recently the investigations of Huxley and others have necessitated, in the present edition, a re-arrangement of the lower divisions of the animal kingdom; the separation of the *Protozoa* from the ciliated *Infusoria*; the abolition of the *Radiata* of Cuvier as an independent sub-kingdom, and establishment of the *Cœlenterata*; and the transference of the classes *Rotifera* and *Cirripedia* into close proximity with the *Crustacea*. For the general reader, as well as the scientific student, Professor Jones's book is perhaps the very best general hand-book for study or for reference; and for the benefit of those who do not already know it, we may briefly indicate its general plan. Commencing at the bottom of the scale, the author takes each class in succession, and after a general description of the class, describes in detail the anatomy of some typical species, including, in the case of the higher forms, the structure of the muscles and nerves, the circulatory and respiratory systems, the generative apparatus, male and female, and the embryology or metamorphoses. With the more important classes, the classification into orders is also given. The illustrations are abundant and really beautiful. To illustrate their completeness, we may mention that in the class of *Annelida*, we have drawings of the dental system, digestive organs, circulatory apparatus, respiratory organs, and generative apparatus of the leech: of the viscera, circulation, sexual organs, and eggs of the earth-worm; besides others to illustrate other families of the order. These engravings would have been still more useful had they been accompanied with some explanation of the scale to which they had been enlarged or reduced; it is somewhat perplexing to find facing one another on opposite pages drawings of *Pulex irritans* and the stag-beetle occupying just the same portion of the page; the beautiful drawing, too, of the head of a flea loses half its value from having no indication of its scale.

Dr. Nicholson's Manual is of considerably smaller size, and is more exclusively a book for the class-room. Pursuing the same general plan, a considerably larger proportionate space is devoted to the higher classes of animals, nearly one-half the volume being given up to the *Vertebrata*. Useful elements which are not found in the larger manual are an account of the distribution of each class in area and in time, together with brief descriptions, in some case accompanied with drawings, of those extinct orders the fossil remains of which so often fill up the gaps between widely differentiated existing forms. The drawings are numerous and good, though not of the same high degree of finish. As a student's book it may be safely recommended.

Notes on the Natural History of the Strait of Magellan and West Coast of Patagonia. By ROBERT O. CUNNINGHAM, M.D., &c.; with Map and Illustrations. Edinburgh: Edmonston and Douglas, 1871.

THE voyages of Mr. Darwin and Dr. Hooker have made us familiar with the main features of the natural history of the southern extremity of the American Continent, to our knowledge of which Dr. Cunningham now adds a useful and interesting contribution. An accomplished naturalist spending the greater part of four years attached to an expedition to that seldom-visited land, could not fail to make many valuable observations and some interesting discoveries, which are here recorded in the form of a continuous narrative, though without much attempt to generalise from them. It is very singular to find in that remote land plants not differing specifically from some familiar European and even British forms, as for instance the sub-alpine *Primula farinosa*, and the celery, *Apium graveolens*, though Dr. Cunningham confirms the observation of Dr. Hooker that the latter loses in Patagonia the acrid pungency which is characteristic of the wild European plant, and becomes mild and agreeable to the palate. The illustrations, on stone, add much to the value of the work, which is a very interesting record of conscientious zoological and botanical labour.

The Students' Elements of Geology. By Sir CHARLES LYELL, Bart., F.R.S., Author of "The Principles of Geology," "The Antiquity of Man," &c. With more than 600 Illustrations on Wood. London: John Murray, Albemarle Street, 1871.

IN 1830, 1832, and 1833 appeared the first edition of "The Principles of Geology," in three volumes, comprising a fourth book, "in which some account was given of systematic geology, and in which the principal rocks composing the earth's crust and their organic remains were described." This fourth book remained a portion of the original work, "The Principles," up to the fifth edition, published in 1837, but was omitted in the following edition, and published by itself in an enlarged form in 1838, under the title of "Elements of Geology." It was subsequently re-cast and enlarged again, and then published in 1851, under the designation of "A Manual of Elementary Geology." After six editions of "The Elements," or "Manual," has been sold, it has seemed good to Sir Charles Lyell again to alter the name of his work, and he now gives forth to the world "The Students' Elements of Geology," which is really the original "Elements" re-written, put into a more compact form, and better adapted to the beginner of the science. By alterations of type and shape, the book is made more handy for frequent reference, whilst space is gained by omitting discussions on theoretical matters, and in some cases by a diminution of the size of the woodcuts. These curtailments, however, are certainly counterbalanced by the addition of new matter, for in place of 520 woodcuts, the new edition contains 636, and for every page of disquisition omitted, certainly a page and a half of new facts is added.

It is, indeed, unnecessary for us at the present time to recommend any work of Sir Charles Lyell's, still less one so thoroughly well known as "The Elements." We need only say that the present edition is specially designed for the student.

Professor Huxley, in speaking of the obligations that the present generation are under to various geological writers of the uniformitarian school, speaks especially of the debt that we owe to Hutton, Playfair, and Lyell; but that, whilst the highly valuable works of the two former are read by but few in the present day, that every page of the latter writer is thumbed by all who wish to make any progress in geological thought. The present edition will go far to increase this inequality—for the readiness of reference and the handiness of this volume will make it, what it ought to be, the constant companion of the working geologist.

There are, besides modifications and additions, improvements in the present volume. Sir Charles Lyell is a man great enough to be able to say, "I have been wrong and others have been right"—in consequence, in deference to the French geologists, he now classes as Lower Miocene what he formerly called Upper Eocene. How many Frenchmen dare make a like concession to an Englishman?

A Dictionary of Science; comprising Astronomy, Chemistry, Dynamics, Electricity, Heat, Hydrodynamics, Hydrostatics, Light, Magnetism, Mechanics, Meteorology, Pneumatics, Sound and Statics; preceded by an Essay on the History of the Physical Sciences. Edited by G. F. RODWELL, F.R.A.S., F.C.S. London: Moxon, Son, and Co., 1871.

"THE number of dictionaries professing to comprehend a general view of the arts and sciences, in a condensed form, already extant is very great; yet great as it is, it is every year rapidly increasing." Thus wrote the editor of a so-called portable encyclopœdia in the year 1826, and verily since his day the number of books of reference has not decreased. One great advantage, however, has resulted from the increased number of topics on which universal dictionaries have to treat; and that is, that no one man can any longer attempt to inform the world upon every subject. It was Dr. Wm. Smith, we believe, who first adopted the plan of inducing a number of other well-known men to write each on the subject with which he was best acquainted, and then the whole were amalgamated into a complete lexicon. The system was found to be very successful, and not only has the original editor continued a series of such works, but others have imitated the design in other branches of knowledge. Among these we may class the work edited by Mr. Rodwell. He has embraced chemistry, astronomy, and the sciences usually called physical. These subjects have been assigned to the following well-known gentlemen:—Messrs. J. T. Bottomley, M.A.; W. Crookes, F.R.S.; F. Guthrie, B.A., Ph.D.; R. A. Proctor, B.A.; C. Tomlinson, F.R.S.; R. Wormell, M.A., B.Sc.; and the Editor; each taking two or three distinct subjects.

The articles in a very brief space give a thorough history of our knowledge on each subject, and bring that knowledge down to the latest discoveries; and besides this they direct the student to the works and papers where fuller information can be obtained. Altogether we can recommend the book very highly both to the student who is desirous of gaining some general information about other sciences than the one to which he especially applies himself, and to the general reader who wishes to keep *au courant* with the scientific knowledge of the day. It is not, of course, from such works as these that the man of science learns the depths of what he requires to know, but there are few so thoroughly well read in all the branches of physical science that they will not find here some knowledge they have partially acquired brought to a focus, and the means of increasing that knowledge suggested to them.

We hope that the somewhat enigmatical notice facing the title page, "that this series is published under the sole direction of J. Bertrand Payne," may mean that the other sciences may obtain like attention to that bestowed upon them by Mr. Rodwell and his coadjutors.

Geology; By JOHN MORRIS, F.G.S., Professor of Geology and Mineralogy, University College, London, &c., and T. RUPERT JONES, F.G.S., Professor of Geology and Mineralogy, Royal Military College, Sandhurst, &c. First Series. London: John Van Voorst, Paternoster Row, 1870.

WE learn from the preface that the intention of the authors is that this work should be considered as a concise guide book, so arranged that it may be useful both to students and teachers of geology. This first series consists of heads of lectures and synopses, with a table of the geological formations in the British Isles. Small as this book is, it is a real *multum in parvo*. It is excellently arranged, thoroughly methodical, and its contents may be readily retained in the memory. On this account it will become an important and valuable assistance to the study, not only of geology, but of what we may term the natural history and physiognomy of the entire mineral kingdom.

Civil Engineers' and Contractors' Estimate and Price Book for Home or Foreign Service. By W. DAVIS HASKOLL, C.E., Author of "Railway Construction," &c. Blackwood and Co.

THIS work is compiled to assist the engineer in preparing estimates for tenders for public works, one of his most important duties, and which affects his professional reputation most directly. It is in two parts, one giving the particulars of price, &c., in matters relating to the estimate, the second part being a priced list of contractors' machinery, plant, and tools. Even the experienced engineer or contractor may have his attention called, upon a glance at this book, to matters which in the haste of estimating might otherwise be forgotten.

The Student's Guide to the Practice of Measuring and Valuing Artificers' Works. By E. WYNDHAM TARN, M.A., Architect. Lockwood and Co.

THIS work, re-edited from Mr. Dobson's "Student's Guide to the Practice of Measuring and Valuing," contains a great deal of fresh matter explanatory to the student of the technicalities and modes of construction employed in the several trades. Mensuration, well-sinking, excavating, bricklaying, carpentry, and masonry; and indeed all the branches of the building trade are fully gone into. The work is well indexed, and arranged to facilitate reference. Tables of constants of labour render the valuing of work done easy to the inexperienced in the trade, and make the book of exceeding use to all who have to deal with their own workmen.

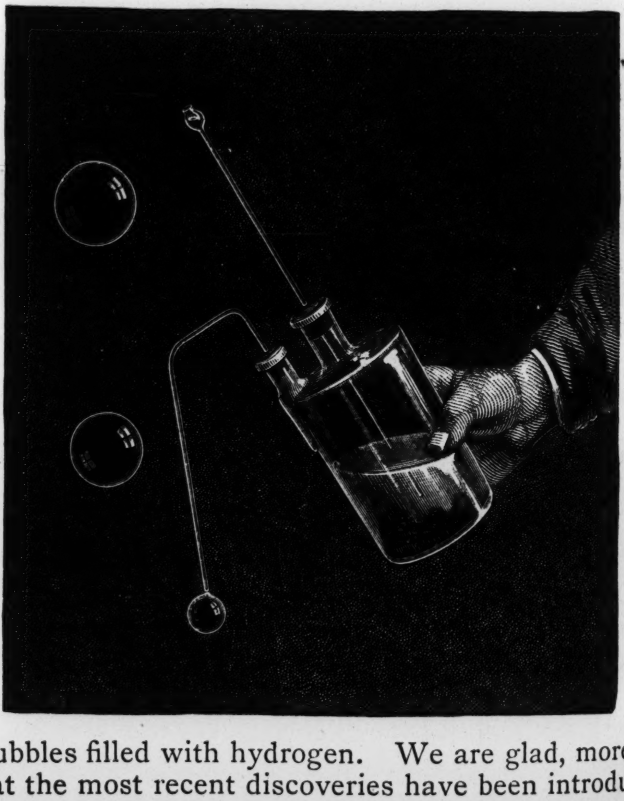
Microscopic Objects Figured and Described. By JOHN MARTIN, Honorary Secretary to the Maidstone and Mid-Kent Natural History Society. Pp. 114; 97 Plates. Van Voorst, 1870.

THIS work consists of a series of 194 lithographs, interleaved with short descriptions of the objects represented, and occasional hints as to mounting, illumination, &c. The objects are represented as filling the field of the microscope, and occupy circles of 2½ inches diameter. The figures are evidently the work of one accustomed to microscopical observation, but the execution is, with very few exceptions, extremely coarse (Fig. 46, seed of *Eccremocarpus*, for instance). The insect illustrations are evidently drawn from preparations made after the usual manner of those who mount objects for sale in balsam, in which muscles, viscera, &c., are carefully removed by alkaline maceration, and little else left but chitinous and membranous structures. In Fig. 129, tongue of the house cricket, the beautiful delicacy of the pseudo-tracheæ is wholly wanting. The proboscis of the blow-fly (Fig. 151) is taken from the popular mutilated and compressed specimen, and for the purpose of perpetuating this object, which has done much to give false views respecting the structure of the oral appendages of the insect, careful directions are given for preparing it. The most useful portion of the work is the short appendix, which contains some plain and simple directions for mounting and preserving objects.

Elementary Treatise on Natural Philosophy. By A. PRIVAT DESCHANEL, formerly Professor of Physics in the Lycée Louis-le-Grand; Translated and Edited, with extensive additions, by J. D. EVERETT, M.A., D.C.L., Professor of Natural Philosophy in the Queen's College, Belfast. In four parts: Part I: Blackie and Son, London, Glasgow, and Edinburgh, 1870. 239 pp. Medium 8vo.

THIS work is the first portion of the translation of M. Deschanel's "Traité Elementaire de Physique," and includes Mechanics, Hydrostatics, and Pneumatics. The complete work was first published by Messrs. Hachette, in 1868, and it has since been adopted by the Minister of Public Instruction as the text book for Government Schools. We have no work in our own scientific literature to be compared with it, and we are glad that the translation has fallen into such good hands as those of Prof. Everett. The type is large and clear, and the woodcuts really admirable, and quite a pattern to scientific works in general, as will be seen from the woodcut given below (Fig. 11), which shows the ascent of soap-bubbles filled with hydrogen. We are glad, moreover, to notice that the most recent discoveries have been introduced.

FIG. 11.*



This portion of the book is divided into eighteen chapters, the first seven of which relate to mechanics, the succeeding four to hydrostatics, then five to pneumatics, and the two last to hydrodynamics.

The work commences with some remarks on the origin of Natural Philosophy. The latter is defined as "the study of the material world, including the phenomena which it presents to us, the laws which govern them, and the applications which can be made of them to our various wants."

Now, we think this definition a little too general, for surely pure physics does not concern itself at all with the applications which may result from the study of its natural phenomena. We think we should carefully distinguish between pure and applied science. Natural science began with the introduction of the experimental method by Galileo (?), and this is defined as a method which consists "in observing facts instead of trying to divine them; in carefully examining what really happens, and not in reasoning as to what ought to happen."

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* We are indebted to Messrs. Blackie and Sons for this woodcut.

In the dynamics we may specially notice the very full and accurate treatment of the "laws of falling bodies," and the detail descriptions of Attwood's machine, Bourbourze's modification of it, and Morin's apparatus. The subject of capillarity receives an unusually full treatment. The book is a valuable contribution to our scientific literature; it will form an admirable text-book for special science classes in schools, and we look forward with pleasure to the appearance of the remaining portions of the work.

Lessons in Elementary Physics. By Balfour Stewart, LL.D., F.R.S., Professor of Natural Philosophy, Owen's College, Manchester. 372 pp. London: Macmillan and Co., 1870.

This work forms the seventh of a series of elementary class books for schools, which already numbers among its authors the Astronomer-Royal, and Professors Huxley, Roscoe, Lockyer, and Oliver.

For the first time, we have an elementary text-book on what we may call *the new physics.* When Francis Bacon introduced his system of philosophy in place of the many middle age systems, which had all, more or less, sprung from Aristotelianism, it was called "the new philosophy." We may, with almost equal justice, speak of the natural philosophy of Thomson, Tait, and a few others, as the new physics; for, compared with the physics of even ten years ago, it is, indeed, in many respects, a new science. A science of units, potentials, energies, and vortices; infinitely more philosophical and absolute than the physics of any previous age.

The arrangement of this work is both original and philosophical. It is distinguished by the lucid style and strong, even grasp which is associated with all Professor Stewart's work and writings. In reading it we have the satisfaction of knowing that it is the work of a man who is very familiar with the experimental portion of the various sciences which he describes, and who is well versed in the more refined methods of research, and in the higher flights of pure inductive reasoning.

We must express some regret that a series of questions is not appended to the work, as it would by this means be rendered more useful as a school text-book. This matter can, however, be easily remedied in a second edition, the speedy appearance of which we may safely predict.

Introduction to the Study of Inorganic Chemistry. By William Allen Miller, M.D., D.C.L., LL.D. London: Longmans, Green, and Co., 1871. 282 pp. Small 8vo.

A short time ago Messrs. Longmans commenced a series of elementary works on mechanical and physical science, "adapted for the use of artisans and of students in public and other schools." This work is the last published of the series, and we are glad to notice that works on the "Theory of Heat," by J. Clerk Maxwell, and on "Sound and Light," by Professor Stokes, are soon to follow.

More than half the work before us is devoted to the chemistry of the non-metallic bodies (ten chapters), while the remaining seven chapters relate to the metals.

There is nothing special or very noticeable in the arrangement or treatment of the subject matter of this work. We all know the thoroughness and completeness of the writings of the late Professor Miller, and to enlarge upon this here is needless. This work is necessarily, to a greater or lesser extent, a condensation of Professor Miller's large "Elements of Chemistry," which is a universally used text-book of the science, and needs no praise. The author was engaged in reading the proof sheets of the work before us at the time when he was seized with the illness which has deprived science of one of its greatest lights, and all who knew Professor Miller of a genial, sincere friend.

A Treatise on Magnetism; designed for the use of Students in the University. By George Biddle Airy, M.A., LL.D., Astronomer-Royal. 220 pp. Macmillan and Co., 1870.

This is essentially a work for university teaching, the subject being treated mathematically, and the mathematics being of the higher order. It was written as one of a series designed to promote and exalt the study of mathematical physics in the University of Cambridge.

The work is divided into twelve sections. The first treats of the dissemination of magnetism through the universe, and it is herein stated that the sun and moon, in all probability, like the earth, act as magnets, although our knowledge of magnetism is limited to the magnetism of iron, steel, &c., and of the earth. A steel magnet is defined as a "bar of steel which, when so suspended or so mounted on a fine point that it can vibrate freely in the horizontal plane, will take a definite direction; and, if disturbed from that direction, will return to it by a series of vibrations gradually diminishing in extent, from the effect of atmospheric resistance, &c." Reasons are adduced for believing that terrestrial magnetism is not produced by magnetic forces external to the earth, and that it does not reside in the earth's crust; three theories are then brought forward to account for the magnetic action of the earth; first, by the action of a small magnet of great power near the centre of the earth (*Mayer's theory*, adopted by Biot and Humboldt); secondly, by the action of two magnets within the earth *(Hansteen's theory)*; thirdly, by supposing that the different kinds of magnetism are distributed irregularly throughout the earth *(Gaus's theory)*. The latter, which is based on very elaborate mathematical calculations, is spoken of as "one of the most beautiful and the most important investigations that has appeared for many years in physical mathematics."

A very interesting account is given under the tenth section of the magnetism of iron ships and the means of obviating its effect upon their compasses. The researches of Mr. Archibald Smith on this subject are here introduced, together with a mathematical investigation of the effect of the ship's heeling.

The importance and value of the series of works on mathematical physics to which this belongs, has been so fully recognised by the universities, that we need say nothing to commend it. Moreover, the Astronomer-Royal—who, be it remembered, has been both Lucasian Professor of Mathematics and Professor of Experimental Philosophy at Cambridge—is capable of treating physical subjects in a manner which is possible to few, and which renders any work from his pen a valuable contribution to the scientific literature of the world.

The Sun: Ruler, Fire, Light, and Life of the Planetary System.

By R. A. Proctor, B.A., F.R.A.S. 8vo., 480 pp. London: Longmans, Green, and Co., 1871.

The interest which the sun possesses for us is manifold. Apart from the fact that he is necessary to our life, and the source of all energy—*i.e.*, of all the work done (save tidal energy) in the world; that in fact, as Mr. Proctor has it, he is "ruler, fire, light, and life of the planetary system," there is a special reason why we should regard our great luminary with the profoundest interest. We may remember that the sun has even been the symbol of the Deity, and (the symbolism being forgotten) has often been himself worshipped. Agni, the god of light and fire, was placed first in the Hindu Trinity, and many of the most charming hymns of the Rig Vedu Sanhita are addressed to him. Then the ancient Persians introduced fire as the symbol of this deity, and Mr. Palgrave, during his late visit to Arabia, found in certain fastnesses traces of the old Sabæan worship still remaining among the inhabitants.

But all this was before man had asked "proud philosophy" to teach him what the sun is; and since nature has been of late communicating her secrets pretty freely in this direction to some well-known astronomers, and the sons of science,—Herschel

Schwabe, Carrington, De la Rue, Balfour Stewart, Huggins, Zöllner, Respighi, Lockyer, Young, &c.,—we must leave the fairy land of a primitive world, peopled by primitive men, with fresh Nature-worshipping intellects, and plunge into the more practical details of spiral protuberances, faculæ, chromospheres, and coronæ.

We are to regard the sun, says Mr. Proctor in the introduction, "as the recognised centre of the solar system, ruler over a scheme of worlds, on which he pours forth abundant supplies of heat and light." The first chapter treats of "the sun's distance and diameter."

The second chapter is entitled "The Sun as Ruler," and herein is discussed the action and influence of the sun as the controlling power of the motions of celestial bodies. At equal distances the sun exerts 315,000 times the attractive power of the earth. "So that if the earth's mass were as great as the sun's, her dimensions remaining unchanged, a mass which now weighs one pound would weigh more than 14½ tons. . . . A body, if raised but a single inch and let fall, would strike the ground with a velocity three times as great as that of the swiftest express train." The mean velocity of the earth around the sun is 18·2 miles per second. At a point as close as possible to the surface of the sun a body would possess a velocity of no less than 378·9 miles per second, while at the distance of Neptune, 2,745,998,000 miles (measured from the centre of the sun), he can control and also generate a velocity of only 4·7 miles per second. We can but be struck here with the wonderful richness of expression which Mr. Proctor possesses, and which he exercises so admirably and so judiciously. Astronomy, which has been ever admitted to be the grandest and sublimest of the sciences, requires such language to give full force to her wonderful results and deductions.

At the close of this chapter we have an example of graphic diction which reminds us somewhat of poor Hugh Miller's description of the six periods of creation, save that the following is more calm and indulges in less lofty flights; it is also less rhapsodical than the generality of Tyndallics.

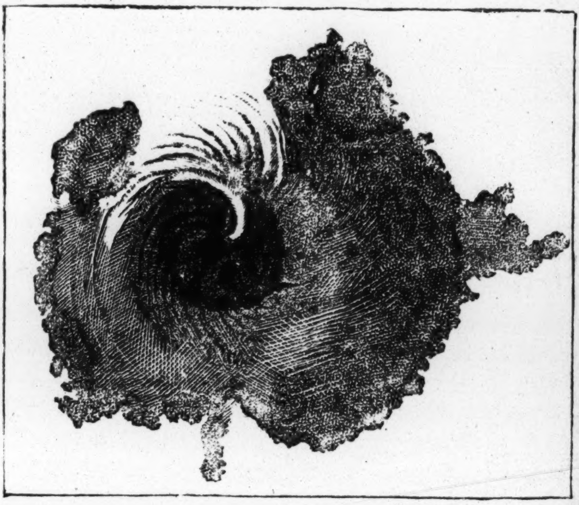
"Tracing back the history of that system, we seem to recognise a time when the sun's supremacy was still incomplete, when the planets struggled with him for the continually inrushing materials from which his substance, as well as theirs, was to be recruited. We can see him by the mighty energy of his attraction clearing a wide space around him of all save such relatively tiny orbs as Venus and the Earth, Mars, Mercury, and the asteroids. With more distant planets the struggle was less unequal. The masses which flowed in towards the centre of the scheme swept with comparatively slow motion past its outer bounds, so that the subordinate centres there forming were able to grasp a goodly proportion of material to increase their own mass or to form subordinate systems around them. And so the giant planets, Jupiter and Saturn, Uranus, and distant Neptune, grew to their present dimensions; and became records at once of the sun's might as a ruler—for without his overruling attraction the material which formed these planets would never have approached the system—and of the richness of the chaos of matter from which his bulk and theirs were alike evolved. Nor is the consideration without a mysterious attraction that, in thus looking back at the past history of our system, we have passed after all but a step towards that primal state whence the conflict of matter arose. We are looking into a vast abysm, and as we look we fancy we recognise strange movements, and signs as if the depths were shaping themselves into definite forms. But in truth those movements show only the vastness of the abysm; those depths speak to us of far mightier depths within which they are taking shape. 'Lo! these are but a portion of His ways; they utter but a whisper of His glory.'"

The third chapter treats of "Analysing Sunlight." The certainty with which we may assert the presence of this or that element in the sun or other heavenly body, is well shown by the fact that no less than 450 iron lines have been recognised in the solar spectrum. Now, the possibility of any such relationship being the result of *chance*, "is less than a fraction whose numerator is unity, and whose denominator consists of no less than 136 figures." The rest of the chapter treats of the application of the spectroscope to the examination of various portions of the sun's surface,—of the faculæ, prominences, &c.

The next chapter gives an account of the various observations which have been made with a view of studying the surface of the sun. Firstly of sun spots, to illustrate which we have two coloured plates, showing, respectively, a portion of the sun's disc observed in May, 1870, and the sun as seen by the author on September 25th, 1870. Faculæ (*facula*, a small torch) were first studied by Herschel, in 1792; he borrowed the name from Hevelius, and applied it to elevated bright places on the sun's surface, an excellent representation of which is given on p. 180. An account is given in some detail of the researches of Schwabe and Carrington on sun spots, and the more recent papers of Stewart, De la Rue, and Loewy. By the application of photography, a record of the appearance of the sun's surface on every clear day is now obtained. No less than 1137 sun spots have been detected. The above-mentioned physicists have, in summing up their recent researches, expressed the following opinion:—"Solar faculæ, and probably also the whole photosphere, consist of solid or liquid bodies of greater or less magnitude, either sinking slowly or suspended in equilibrium in a gaseous medium. A spot, including both umbra and penumbra, is a phenomenon which takes place beneath the level of the sun's photosphere." Several admirable woodcuts of sun spots are given in this chapter, notably one (Fig. 48) which represents a group of spots and the solar willow leaves. Mr. Dawes was the first to detect the appearance of cyclonic motion in a spot, as if a solar storm were taking place there; this is shown in the following figure (fig. 12).

The dimensions of sun spots are enormous. In 1858 a spot was noticed which was 107,520 miles long; and, in the same year, the largest spot ever recorded was first noticed:—"It had a breadth of no less than 143,500 miles; so that across it no less than eighteen globes as large as our earth might have been placed side by side."

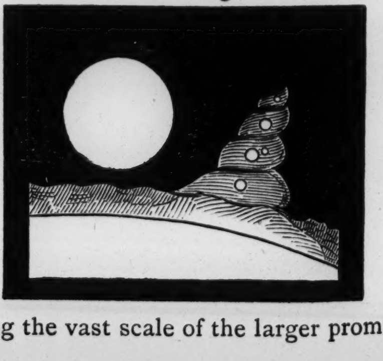
FIG. 12.



A spot presenting the appearance of cyclonic motion. (*Secchi.*)

The prominences and the chromosphere form the subject of the following chapter. The former were first examined during the total solar eclipse of 1842, but were perhaps detected in 1733, by Vassenius, of Gottenburg. Several subsequent observations were made; the most important, until recently, being those of Mr. De la Rue and Father Secchi, in 1860. The prominences were then proved to be real phenomena belonging to the sun, and to possess great luminosity, brilliant colour, and considerable photographic power.

FIG. 13.

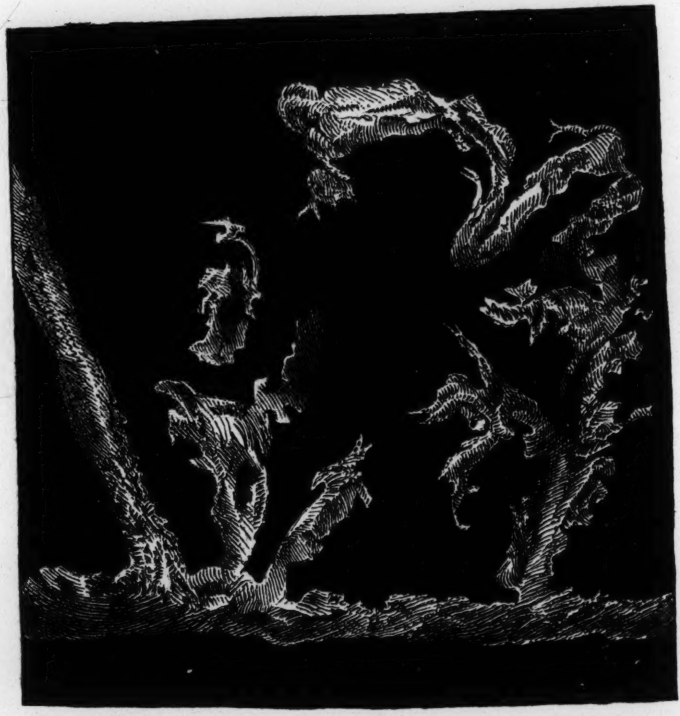


Illustrating the vast scale of the larger prominences.

Secchi further noticed that there were "masses of prominence matter suspended and isolated like clouds in the air." The height of the solar prominences is enormous:—130,000 miles is considered by astronomers no exaggerated amount. The following figure illustrates the great altitude of the larger prominences very strikingly.

The large disc on the left of the prominence represents the comparative dimensions of Jupiter, while the uppermost of the small discs within the prominence represents Mercury, the next Venus, then the Earth and Moon, and the lowest Mars. A coloured plate (p. 272) represents the prominences as seen during the total eclipse of August, 1868. The following figures represent a group of prominences drawn by Mr. Lockyer, and the group only ten minutes later. Dr. Zöllner has also figured a number of prominences, the most interesting of which are represented in beautifully coloured plates. Professor Respighi has studied the form and nature of the prominences even more fully than Zöllner.

FIG. 14.

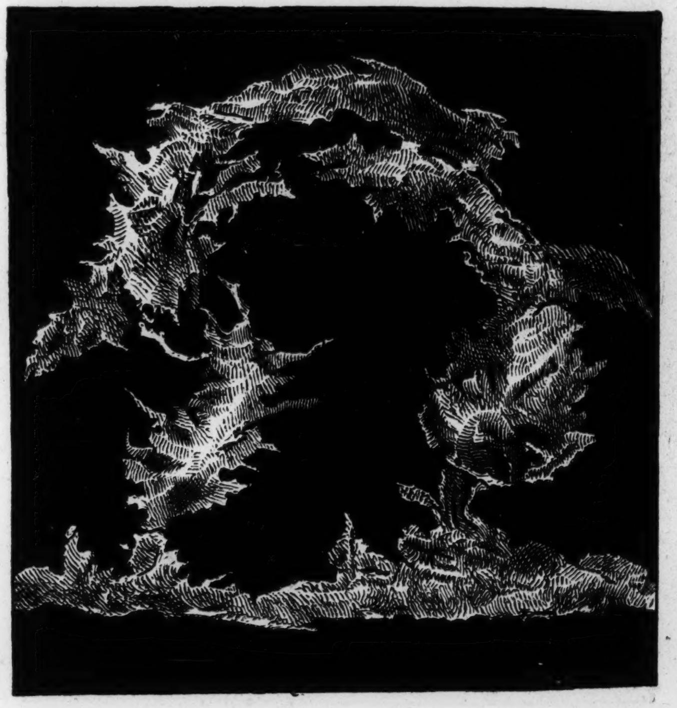


A group of solar prominences.—March 14, 1869, 11h. 5m. (*Lockyer.*)

Chapter 6 treats of "The Corona and Zodiacal Light." The corona is the crown of light which surrounds the black disc of the moon during a total eclipse; it is first mentioned by Apollonius, and later by Plutarch. A detail notice is given of eclipses in which remarkable coronæ have been observed, the most interesting observation having perhaps been made in 1842 by Arago.

In the next chapter the "Physical Condition of the Sun" forms the subject of study, and this is followed by a short chapter entitled "The Sun, our Fire, Light, and Life." The heat, light, and chemical activity which we receive from the sun has of late been often and accurately estimated. As to the heat, it has been calculated that if the sun's heat were distributed uniformly over the surface of the earth it would in one year suffice to liquefy a layer of ice 100 feet thick; and this amount of heat, be it remembered, is only the $\frac{1}{2,138,000,000}$th part of the heat emitted by the sun in the course of one year. The absolute luminosity of the sun's surface is more than 146 times the luminosity of the lime light. The subject of the dissipation of energy and the maintenance of the sun's heat finds ample discussion in this chapter.

FIG. 15.



The same group as shown on the last page ten minutes later.

The last chapter is entitled "The Sun among his Peers." The sun, as is well known, possesses proper motion of his own; he is carried forward in space, and with him all the members of the solar system which revolve around him. We do not know, however, whether any of his brother suns accompany him. As the earth is carried along with the sun, while it rotates around him, the path followed by it is a helicoidal path.

"Jupiter is carried some 1,700,000,000 miles onward with the advancing sun, while he circuits once around his orbit of less than 1,000,000,000 miles in diameter; Saturn sweeps on through some 4,400,000,000 miles while circuiting his orbit, less than 1,850,000,000 miles in diameter; and the paths traversed by Uranus and Neptune amid the depths of sidereal space are even more remarkably drawn out, regarding them in their helicoidal character."

The work is terminated by two appendices; the one relating to "the approaching transits of Venus, and the best means for observing them;" the other to Eclipses. By means of the transits of Venus we can obtain, with greater accuracy than by other means, knowledge as to the sun's distance from us,—it is needless to say a matter of the extremest importance. The next transit will take place in 1874, and the Government has voted a sum of £10,000 for the purposes of the observing expeditions. Two maps accompany this appendix, and it contains many very useful hints as to the most effective method of observing it completely. There will be a second transit in 1882.

We have endeavoured to give some insight into a book which commends itself both to the man of science and to the general reader. The complete astronomer must revel in this book, not less than the man who, when he opened it, did not know the meaning of parallax or proper motion, and who was all ignorant of solar willow leaves and spiral prominences. The most recent results have been introduced, yet the book is popular in style. Popular scientific books are seldom the work of the real *savant*, yet Mr. Proctor is an accomplished astronomer and sound mathematician, and scarce a number of the "Journal of the Royal Astronomical Society" appears without a paper from his pen. We have long wanted books in this country of the *Le Ciel*, and *Phenoménes de la Physique* of the Guillemin class: popularly written, well printed, profusely illustrated, and accurate withal; instructive without being dull; stimulative and of a nature to lead the reader deeper and deeper into the depths of science. Mr. Proctor has supplied us with such a book as regards astronomy, and we cannot be too grateful to him.

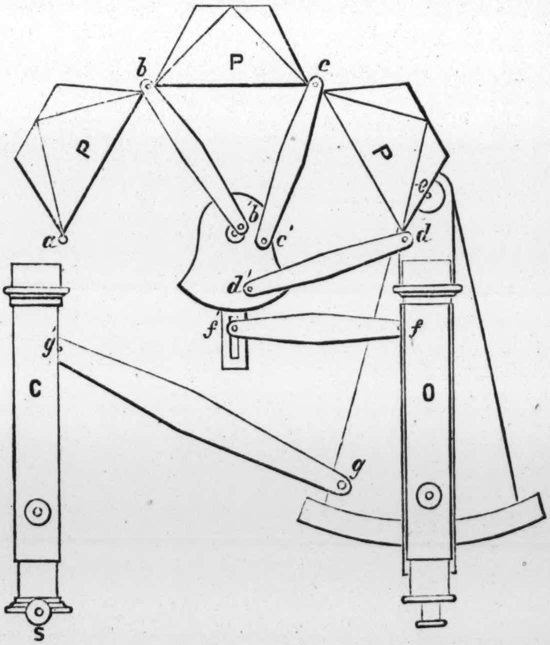
PROGRESS IN SCIENCE.

LIGHT.

Grubb's Automatic Spectroscope.—P, P, P are three prisms placed, we will say, in the position of minimum deviation for some mean ray—O being the observing telescope, and C the collimator carrying the slit at S. The prism tables are jointed together in a species of chain; the first point, *a*, of the chain working on a fixed stud let into the base of the instrument; the other three joints, *b*, *c*, and *d*, are connected by steel levers with three studs, *b'*, *c'*, *d'*, in a central disc attached at different distances from the centre, and in such positions that the levers form tangents in their mean positions.

If the distances of the studs from the centre be properly proportioned, the rotation of the disc draws the three joints, *b*, *c*, *d*, in or out as the deviation is required less or more, in just such a ratio as will preserve the position of minimum deviation in all the prisms. So far for the prisms; and now as to the telescopes. The collimator is stationary, pointed at the centre of the first surface of the first prism, which hardly moves at all. The observing telescope is attached to an arm which is centred on a point, *e*, in the third prism table, which corresponds to the centre of the last surface of the third or last prism, so that in whatsoever manner the telescopes or prisms are turned, this telescope always points towards the centre of that surface, and consequently takes in the whole pencil of light emerging from that surface. On the same pivot is centred also a sector to measure the angle through which the observing telescope is turned.

FIG. 16.



The arm to which the observing telescope is attached is coupled to the central disc by a connecting rod, *f*, *f'*, of such proportions that the movement of the telescope round its pivot for the purpose of viewing the different parts of the spectrum will also rotate the disc and cause the prisms to assume their proper positions for that particular refrangibility of light under examination.

And, finally, a point, *g*, in the limb of the sector is coupled to a fixed centre, *g'*, in the bed plate by another connecting rod, *g*, *g'*. This is for the purpose of compelling the sector to travel in a direction parallel to itself; that is to say, if in any position of the instrument a line were drawn upon the sector parallel to the collimating telescope, it would also be parallel to it in every other position. Hence, a little consideration will show that the readings of the sector give actual differences of deviation.

In fact, if the two telescopes were placed parallel to each other, and that point marked 180° on the sector and the other parts numbered up and down from that, the instrument would then read actual deviations.

In practice, however, such a system of division will probably not be found as convenient as some others, as it is only differences that are required; but these differences, it will be seen, will not be correctly given unless the sector has that particular motion imparted to it described above.

Two-Prism (Compound) Spectroscope on Automatic Principle.—The two-prism spectroscope for Dr. Huggins's star observations is constructed exactly on this same principle, modified, of course, to suit the two instead of three prisms. The prisms are compound, and admit of a pencil of light, 1¼" × 1" to pass to objectives of telescopes which are 1¼" diameter. The power of the spectroscope is, therefore, equal to that of four large prisms of 60°. There is one common collimator used for several spectroscopes; this collimator is 1¼" aperture and 4½" focus. The great shortness of focus is obtained by making the object-glass of that quadruple form which Mr. Grubb applied to the 7-inch circle telescope in Armagh. The advantage of having the collimator of large angular aperture is that a more powerful cylindrical condensing lens can then be used and greater brilliancy obtained.

Compound Prisms.—The compound prisms devised by Mr. Grubb, F.R.S., for spectroscopic investigation are composed of three elements. The centre one being of dense flint glass of an angle varying from 90° to 100°, according to the exact nature of the glass, and crown prisms cemented on each side of about one-fourth that angle reversed in the direction of their apices. By cementing these crown prisms on the centre flint, the light which otherwise would not be capable of passing either in or out of the prism is enabled to do so, and a dispersion is by this means obtained equal to about two 60° prisms, and with a deviation equal to about one 60° prism. Various prisms have been made from time to time on the compound principle, and by various artists, but in most cases they have been tied with the condition of "direct vision," which is by no means necessary, except for special purposes, and sometimes even undesirable.

In other cases the centre flint has been made of extravagant angle; this is also useless, as crown prisms of great angle must then be attached, which again brings down the dispersion; and the inside surface being so much inclined to the course of rays much light suffers internal reflection. In the proportion arrived at by Mr. Grubb, the required qualities have been found both theoretically and practically to be the best balance. The advantages in using them can be summed up as follows:—Double the dispersion is obtained by them as compared with an equal number of ordinary 60° prisms with the same number of surfaces (the inside not counting, being cemented), a less absorption from the glass, one-half the complication of mechanism (particularly if automatic spectroscopes be used), and freedom from danger of injury to surfaces of flint glass.

Mr. Browning has contrived a direct vision spectroscope, for use in the Bessemer process. It has great dispersive power, with very low magnification, the result being a very luminous spectrum, which fills the whole field of view. The jaws of the slit are opened or closed by turning a milled ring just behind them. There are no adjusting screws or movable parts to this instrument. It is made with what are known as fixed adjustments, so that in an ordinary workman's hands there is nothing to get out of order. The instrument contains ten prisms, and will divide the D lines in the solar spectrum quite easily.

Mr. Browning has recently published some chromo-lithographs of spectra for the special purpose of keeping records of the position of absorption-bands as observed with his micro-spectroscope. The size of the paper is the same as that of the present journal, and each plate contains seven coloured figures on a black ground, with the places indicated of the principal solar lines; the memoranda can be made with pencil, or, still better, with a brush, and a tolerable representation of the observed spectrum obtained. This mode of registration will be preferred by many to Mr. Sorby's ingenious notation as adapted to his interference spectrum, an instrument somewhat difficult to procure, on account of the trouble involved in making the plate of quartz of the proper thickness to produce the exact number and position of the bands.

Dr. Huggins has devised a registering spectroscope, by means of which the positions of lines observed in the spectrum may be instantly registered without removing the eye from the instrument, so as to avoid the loss of time and fatigue to the eye of reading a micrometer-head, or the distraction of the attention and other inconveniences of an illuminated scale. In this instrument the small telescope of the spectroscope is fixed, and at its focus is a pointer which can be brought rapidly upon any part of the spectrum by a screw-head outside the telescope. The spectrum and pointer are viewed by a positive eye-piece which slides in front of the telescope, so that the part of the spectrum under observation can always be brought to the middle of the field of view. The arm carrying the pointer is connected by a lever with a second arm, to the end of which are attached two needles, so that these move over about 2 inches when the pointer is made to traverse the spectrum from the red to the violet. Under the extremity of the arm fitted with the needles is a frame containing a card, firmly held in it by two pins which pierce the card. This frame containing the card can be moved forward so as to bring in succession five different portions of the card under the points of the needles; on each of these portions of the card a spectrum can be registered. The mode of using the instrument is obvious. By means of the screw-head at the side of the telescope, the pointer can be brought into coincidence with a line; a finger of the other hand is then pressed upon one of the needles at the end of the arm which traverses the card, and the position of the line is instantly recorded by a minute prick on the card. From ten to twelve Fraunhöfer lines can be registered in about twelve seconds, and when the same lines are recorded five times in succession on the same card, no sensible difference of position can be detected between the pricks registering the same line in the several spectra.

A most ingenious application of the spectroscope, and one likely to be of considerable use in many enquiries, has been made by Professor Church. On one side of a crowded court several cases of typhoid fever had been developed. The water used by the inhabitants of these houses was drawn from a rather shallow well, and was highly charged with various unoxidised compounds of nitrogen. It was suspected that the drain from a public urinal might be defective and have allowed egress of its contents into the well. This notion was confirmed by the quantity of common salt contained in the well-water, namely, seven times as much as that in the normal waters of the neighbourhood. But it received an absolute proof in the following novel manner. Two grammes of a *lithium* salt were introdued into the urinal. Two hours afterwards lithium was detected spectroscopically in a litre of the well-water before alluded to. A quantity of this water, ten times as large, showed no trace of lithium previously.

MICROSCOPY.—Dr. Ormerod, of Brighton, uses a new material for grinding sections of bone, tooth, and similar hard tissues. The bone is first cut into slices with a saw in the usual manner, and rubbed down to an even surface on a file or coarse stone; then, using a piece of flat pumice stone as a pad, it is rubbed down with water to the necessary degree of thinness on a piece of coarse ground glass about 6 inches square; the glasses, when the first roughness is worn off, may be used for giving a still finer surface to the section. Ground glass will be found to act rapidly and efficiently as a grinding agent upon tissues of the hardness of those mentioned. The section, when dried with blotting paper, but *still moist*,* is to be mounted upon a slide on which Canada balsam has been hardened, and covered with a thin glass on which balsam has been similarly hardened, the moist surface prevents the balsam penetrating the lacunæ and canaliculi and obliterating the structure. With practice, from the time of cutting the slice till the slide is ready for the cabinet, about half an hour will have elapsed.

A machine for grinding sections of hard substances for microscopic purposes is described by Mr. C. Sellers, in the "Proceedings of the Academy of Natural Sciences of Philadelphia." It consists of an emery or corundum wheel similar to those employed by dentists, over which is placed a plate having a circular aperture, through which a portion of the edge of the wheel projects; the amount of this projection is capable of being altered by means of a screw adjustment. The substance to be ground is attached by a suitable cement to a glass slide and moved over the aperture while the wheel is rotating until a level surface is obtained. The contrivance is ingenious, but for producing perfectly plane surfaces nothing can equal the lap or horizontal wheel commonly used by opticians and lapidaries; the edge of a wheel has a constant tendency to grind a hollow place, and most careful shifting of the object is required to secure any approximation to truth.

Dr. Barker brings before the Royal Irish Academy an adaptation of the immersion principle for the illumination of microscopic objects. The good qualities of hydro-objectives or immersion lenses, with respect to improved definition, increase of light, working distance, and magnifying power, are already well known; corresponding advantages are to be obtained by the use of illuminators constructed on a similar plan. Dr. Barker has at present only experimented on the principle as applied to Wenham's paraboloid, which he constructs with a flat top (Fig. 17), instead of the usual hemispherical cavity, and introduces a film of water between it and the under surface of the slide, thus securing optical contact between the paraboloid and the slide; the film of fluid will also act as a water joint, and allow free action to the stage movements, so that any part of the slide can be easily examined. The oblique rays are thus economised; little dispersion takes place if the object is mounted in fluid or balsam, and there is sufficient brightness for all powers. Another advantage arises unexpectedly, for if the focus of the paraboloid be made a little higher, or if a slide of extra thinness be used, the oblique rays will undergo total reflection from the upper surface of the covering glass and be sent down on the object so as to illuminate it by reflected light

FIG. 17.

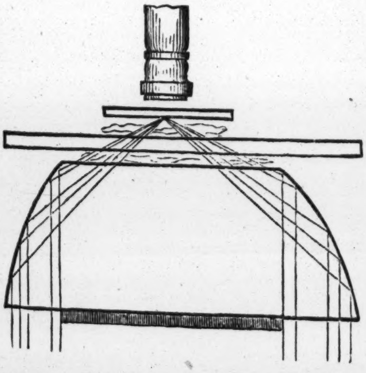
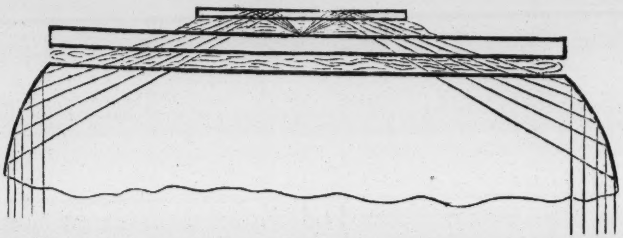


FIG. 18.



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* Mounting a specimen damp in balsam must sooner or later give rise to a cloudy appearance in the surrounding medium; if the balsam is well hardened and used with the smallest necessary amount of heat, there is but little fear of the canaliculi being filled.

(Fig. 18), in a nearly similar manner to Mr. Wenham's combination of paraboloid and truncated lens,* but of course with less loss of light. The following constructive details are given. If the focus be chosen $\frac{1}{12}$th of an inch above the upper surface, the equation of a perpendicular section $y^2 = ax$ will become $y^2 = a\left(\frac{a}{4} + \frac{1}{12}\right)$; and solving the equation for a, we shall have $a = -\frac{1}{6} \pm \sqrt{4y^2 + \frac{1}{36}}$; and if the upper surface of the paraboloid be made $\frac{3}{4}$ths of an inch in diameter (a size most convenient in practice), a becomes $=$ to $\frac{3}{4}$, and the equation $y^2 = \frac{3}{5}x$ is that of the paraboloid form. Should the focus be taken at a distance of $\frac{1}{10}$th of an inch above the truncated paraboloid, and its upper surface be 1 inch in diameter, then the equation becomes $y^2 = \frac{3}{7}x$ *quam proxime:* this latter form will admit of a hollow cone of light of 120° to 185°, and will almost give a dark ground illumination for a $\frac{1}{8}$th immersion object glass. A modification of the same principle is also given by which objects may be illuminated from above and the use of the highest powers permitted. Dr. Barker further considers that the immersion principle is applicable to all kinds of condensers placed in the axis of the microscope.

The thinnest kind of glass used for covering microscopical objects is very troublesome to clean on account of its brittleness; when slight friction will remove the dirt it is best rubbed between two discs of wood or metal covered with wash leather, but if cleaning with alcohol or other fluid is needed, accompanied with somewhat hard rubbing, the breakage is considerable. The fluid used by photographers for cleaning glass plates, composed of sulphuric acid 1 oz., bichromate of potassium 1 oz., water 1 pint, will be found very effective. The thin glass is to be left in this solution for some hours,—a day or two will do no harm; it can then be poured off and the glass washed with several waters to remove all traces of the preparation, the last washing being with distilled water. The thin glass is placed upon a soft porous cloth† spread upon a table and carefully rubbed dry with another cloth; it will be found that very little breakage takes place, as the glass is so perfectly cleaned even when extremely dirty or covered with *bloom*, that very little pressure or hard rubbing will be needed. This process may be used with advantage for cleaning slides as well as cover glass; the solution is cheap, and effectually cleanses all kinds of glass with the minimum amount of trouble.

Dr. Ward, at the American Association for the Advancement of Science, remarked "that the production of a beaded appearance on the Podura scale as a purely optical effect should be considered no longer doubtful, but rather as an occasional accident to persons using high powers." As an extreme instance in the case of a coarse and familiar structure, he related that, while experimenting upon an elater of *Marchantia polymorpha*, that beautiful double spiral was resolved into three rows of "beads" or "hemispheres," perfectly distinct and unmistakable, which occupied of course the position of the middle and edges of the spiral. They were illuminated by parallel light, very oblique, under a $\frac{1}{15}$th objective of 175°, worked at a power of 3000 diameters.

According to the "American Naturalist," October, 1870, Colonel Dr. J. J. Woodward has succeeded in photographing the beaded appearances in the scale of *Degeeria domestica*, from specimens supplied by Mr. S. J. McIntire. They are plainly seen in a series recently received by the Royal Microscopical Society: also the lines of *Amphipleura pellucida* (*Navicula acus*), and the hemispheres of *Surirella gemma*. Mr. Wenham considers the beads as "ghost beads," and evidently caused by intercostal corrugations of membrane.

Mr. T. H. Hennah, of Brighton, has succeeded in procuring a series of photographs of arrangements of intersecting glass rods, as suggested by Dr. Pigott: the results are curious in the extreme; they were placed before the writer without his being informed of their nature, and were at once pronounced to be diatom hemispheres, insect eyes with diminutive images of objects, and something very like the ! markings of the Podura test scale, *Lepidocyrtus curvicollis*. The appearances in the photographs are much more deceptive than those seen in the glass rods themselves, as the observer is aware of what he is looking at. The effect is most conveniently produced by mounting a number of small glass rods placed in contact in two circular frames, which are caused to revolve over each other by the contrivance used in chromatrope slides. The deceptive appearances are very well shown by using the slide as a magic-lantern object.

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* Monthly Microscopical Journal, 1869, vol. ii., p. 28.

† Old cotton stocking, carefully cleaned with soda and hot water to remove the soap from former washings, answers well for this purpose.

The fossil sponge spicules obtained by Mr. W. Vicary, of Exeter, from the greensand of Blackdown and Haldon, and described by Mr. E. Parfitt in the "Transactions of the Devonshire Association for the Advancement of Science," have been further examined by Mr. H. J. Carter, F.R.S., and form the subject of a paper in the "Annals of Natural History," vol. vii., p. 112. The spicules are embedded in quartz sand, and are, as Mr. Carter believes, the remains of dead and disintegrated sponges, consisting chiefly of the larger spicules, more or less in a fragmental condition, and altered in shape by trituration; the minute forms of spicules are generally absent. The surface of the spicules is considerably eroded, and presents the peculiar form of chalcedony. The triturating and solvent influences attending petrifaction have probably obliterated any spinous or tubercular processes which might have been expected to have been found on spicules of so large a size, and which exist on recent sponge spicules with which they are identical. Besides sponge spicules, the deposit contains a few minute bivalve shells and *Foraminiferæ*, but no remains which could be identified with the calcareous spicules of *Echinodermata*, *Alcyonidæ*, *Gorgonidæ*, or *Ascidiæ*. Owing to the tolerably perfect condition of many of the spicules, Mr. Carter has succeeded in identifying a large number of them; but owing to the almost entire absence of the smaller spicules, it has only been possible to make the comparisons with the larger spicules of recent sponges. The four plates which illustrate the paper contain seventy-six figures of spicules from the localities named.

Mr. H. J. Slack has noticed the unusual form of microscopic crystals produced by dissolving various salts in a colloid solution of silica obtained by dialysis. Sulphate of copper afforded the following results:—First, what may be termed a "pavement pattern," in which the silica cracks divide the film into a great number of compartments, of three, four, five, or more sides, which may be roughly likened to the appearance of vegetable ivory, or a section of pinna-shell. In each of these divisions crystallisation takes place, usually radiating from the centre, and sufficiently diversified in thickness to produce polariscope patterns. The crystals thus produced have a tendency to exhibit Maltese crosses, of colour changing with the positions of the polarising and analysing apparatus. In these "pavement patterns," the force exerted by the silica film in contracting and cracking causes the crystalline force to operate in limited compartments without change in the linear direction common to radiating groups. In other cases, the contractile force acts tangentially, or in curves, while the crystalline force operates radially, and then exquisite spiral and turbinate forms, similar to those described by Mr. R. Thomas (Quart. Journ. Micr. Sci., 1866, p. 177). Hippuric acid, salicine, and tartaric acid, all yielded patterns differing from the ordinary forms of crystallisation from aqueous solutions. Mr. Slack remarks, in conclusion, that it is impossible to view a number of slides prepared in this way without being struck with the resemblance of many of the patterns to those of sections of various structures in organised beings, and we are led in the direction long since indicated by Mr. Rainey, to extend our notions of the functions performed by the chemical and physical forces at work according to their own methods in living things. The action of the silica appears to be chiefly physical and mechanical; but the appearance of the crystals leads to the belief that some portion of silica is present in them, and is not eliminated as the process of crystallisation goes on. (Monthly Micro. Journ., vol. v., p. 50, and pl. 77 and 78).

A new microscope lamp, designed by Mr. Fiddian, of Birmingham, has recently been brought forward by Mr. Browning. One of its merits is great portability, the size of the cylindrical case, Fig. 19, in which it is packed being only 6 inches by 3 inches, and its disposition when put away will readily be understood by Fig. 20. The chimney is of metal, and to enable it to be placed in so small a case, the upper part consists of two joints sliding into each other like the drawers of a telescope; the aperture in the bulb is furnished with white and tinted glasses, and also with a plano-convex lens for obtaining parallel rays. The general form of the lamp is shown in Fig. 21. An arrangement is made which permits the lamp to be tilted to a slight extent when required. The burner is small and of good quality, and the intensity of the light much increased by the white lining of the interior of the chimney, which can easily be renewed whenever it becomes discoloured by the deposition of soot. Mr. Browning is engaged in making provision against the liability of spilling the paraffin in case of the accidental inversion of the lamp during conveyance from place to place, an improvement that will be fully appreciated by microscopists who are in the habit of using their instruments away from home.

FIG. 19.

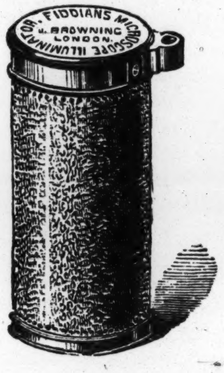


FIG. 20.

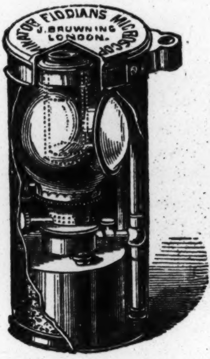
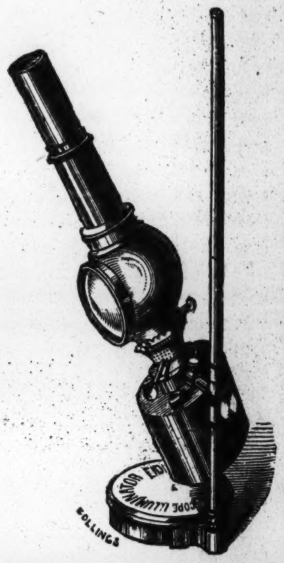


FIG. 21.



Captain F. H. Lang, President of the Reading Microscopical Society, communicates his experience, and that of Captain Haig and Mr. Tatem, respecting the selection and mounting of diatoms. The instrument preferred by Captain Lang for picking up diatoms is a finely pointed badger-hair, whipped on to a light handle, so that the hair does not project more than about ⅜ths of an inch. This is dipped into a weak solution of gum and allowed to dry, and, when breathed upon, the most delicate form from a dried gathering may be taken up, and will remain on the hair whilst the slide containing the general gathering is shifted, and that on which it is to be placed substituted. Captain Haig uses a thin cell, formed of gold-size, baked until it is partially carbonised after the manner of japanning, to prevent the diatoms being crushed by pressure. The diatoms are to be mounted on the cover in preference to the slide, on which is placed a smear of glycerine, containing a minute quantity of gum to keep the diatoms in their places. The glycerine is then disposed of by evaporation on a hot plate, and the diatoms mounted either dry or in balsam, or the solution of balsam in benzol or chloroform. Mr. Tatem places his cover, on which the diatoms have been arranged, on the cell which has been filled with chloroform, and places a drop of balsam in contact with the edge of the cover, allowing it to run in as the chloroform evaporates. For keeping the diatoms in their places when gum is necessary, a grain and a half in an ounce of distilled water is generally sufficient. A small drop of this fluid is preferred by some to arrange their diatoms in instead of glycerine.

Mr. F. H. Wenham contributes a valuable paper on "Object-Glasses and their Definition," to the January number of the "Monthly Microscopical Journal." It is an admirable sequel to his series of papers on the construction of object-glasses, vol. i., p. 111, &c., of the same journal. After criticising the papers of Dr. Pigott,* and calling attention to the fact of the reduction of angular aperture of the objective, when used on objects mounted in Canada balsam, demonstrated by himself and Professor Robinson in 1854,† he proceeds to explain, by the aid of a diagram on a large scale, the course of the rays through an eighth objective of 130° aperture of his own construction, and giving reasons for the various combinations of lenses employed in it. Some remarks are made respecting the vexed question of the structure of the Podura scale, and the appearances shown in Colonel Dr. Woodward's photographs defended against Dr. Pigott. Mr. Wenham bears testimony to the extreme value of the "mercury globule" as a test for ascertaining the nature of the aberrations of object-glasses during their construction, and gives some details of the mode of reading its delicate indications. The subject of immersion lenses is treated in a very plain manner, and their effects illustrated by large diagrams. Of the one described, he writes as follows:—"The effect of this immersion lens is to give greater clearness and brilliancy to the object, and render markings more distinct that were before scarcely visible with the dry lens. This is, in fact, attributable to the saving of light and comparative absence of refraction and reflection from the top surface of cover and front of lens; but the great merit consists in the perfect correction that the adjustable thickness of the water stratum affords in compensating for every thickness of cover. Nor is the thickness of an immersion front a matter of particular nicety, for it can be made as thin as desirable; the water will occupy the place of the deficiency." The whole paper is one of great interest at the present time, when the subject of high power definition is being actively discussed.

HEAT.

The following criticism of a paper by the Rev. H. Highton has been forwarded to the Editor by John Hopkinson, D.Sc.:—Mr. Highton's first chapter is devoted to the consideration of the heat produced in the various parts of a conducting circuit by the passage of an electric current. The paradoxes there proposed seem to be founded on a misconception as to what the received views on the subject are. He assumes that the heat generated in a voltameter, or battery cell, follows the same law as in a metal wire—that it is proportional to the resistance and square of the intensity, and is independent of the chemical decompositions which may take place; from this, by reasoning which involves further assumption, he shows that the energy produced by a given consumption of zinc is variable. To show that he is mistaken about the facts, I will quote from Jamin's "Cours de Physique," vol. iii., p. 173:—"M. Becquerel admits that it (Joule's relation between heat and intensity of current) is applicable when we electrolyse sulphate of copper with copper electrodes, because, though on the one hand, there is solution of metal, and consequently absorption of heat at the positive pole, on the other there is a deposit of an equal quantity of copper and a disengagement of the same quantity of heat at the negative pole. Everything goes on as if there were no decomposition, and the law, heat $= Kri^2$, applies to this liquid as to a solid. The circumstances are totally different when the voltameter considered contains water with platinum electrodes. The gases, as they are disengaged, absorb all the heat which they produce in combining; it is proportional to their quantity; that is, to the intensity, i, and a constant, E. Consequently, the heat found in the voltameter ought to be equal to $Kri^2 - Ei$. Experiment confirms this explanation." It will be seen, then, that the fundamental assumptions of the first chapter are contradicted by the facts of the case, as explained in an elementary manual. It

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[*] Monthly Microscopical Journal, July and September, 1870.

[†] Quart. Journ. Micr. Sci., July, 1854, p. 212; and January, 1855, p. 165.

is, therefore, useless to discuss the reasoning until these are proved false. The rest of the paper refers more immediately to the conversion of heat and chemical action into mechanical effect. It will be amusing to anyone conversant with the subject to observe how, in the second paragraph, Mr. Highton gravely sets forth the elementary deductions of thermodynamics as anomalies. That stretching a wire, within the limits of its elasticity, cools it, follows from the fact that heat expands it. A little further on, we find a confusion of energy with the *availability of energy*, or, as Tait calls, the *eutropy*: "There is just as much mechanical energy in a lump of ice which will produce 100 units of cold as there is in a lump of coal which will produce 100 units of heat; there is as much stored up power in a glacier as in a coal-mine." . . . "No amount of heat in a body can produce any effect till that body comes into contact or communication with some other body either hotter or colder than itself." If Mr. Highton will refer to Professor Tait's "Thermodynamics," or Professor Balfour Stewart's "Heat," for the second law about reversible engines, he will see in what way a cold refrigerator is not exactly a source of mechanical energy, but a means of converting more energy already existing as heat into the form of visible motion than we otherwise could; the glacier Mr. Highton proposes to use would be not a source of energy itself, but might serve to render the energy existing as heat around us available. He proceeds: "So that, in reality, force is produced, not by heat or cold, but by the restoration of equilibrium in the heat of two bodies, or parts of bodies, unequally heated, and mechanical energy produces neither heat nor cold (except accidentally), but simply a disturbance of the equilibrium in the heat of two bodies, or parts of a body." I may just point out that we are not talking about force, but about energy or work. I admit we cannot transform heat into force any more than into yards; there is no more an equivalent of heat in pounds weight than in pints, though there is in foot-pounds; but a foot-pound is not a force. This confusion of work and force is common in Mr. Highton's writings. It produces a serious error in his paper in the "Chemical News," of January 27th, which is repeated in the note at the end of the paper in last quarter's "Journal." He then assumes that, if the forces in operation in an engine are greater, the engine will necessarily produce more work from the same quantity of fuel. But I presume that, in this case, though the reverend gentleman says force, he means work. Now, though to change heat into work, we need to have a source and a refrigerator of different temperatures, yet, of the heat which leaves the source, only part ever reaches the refrigerator, the difference disappears as heat, and is changed into the equivalent amount of work. What is meant by mechanical energy producing heat *accidentally*, I fail to comprehend. Possibly, when the theory which shall replace the science of Thermodynamics is set forth, all cases of failure will be referred to the chapter of accidents, and the theory will have mainly to be proved by its exceptions. But the fact that mechanical energy may be transformed into heat was proved long ago, beyond dispute, by Davy's well-known experiment, in which he melted two pieces of ice by rubbing them together in an atmosphere itself below the freezing point. In paragraph 7, we have—"A given amount of heat applied to expand air will raise ten times the weight that it will if applied to expand vapour of turpentine, and one and a quarter times as much as if it were applied to expand steam. It may be answered, 'Yes; but it also expands the vapour of turpentine or water, as well as raises the weight!' True; but this is not mechanical energy as measured by foot-pounds raised; and to assume that it is equivalent to it, is to beg the question at issue." I think Mr. Highton makes more than one mistake here. His premises are loosely stated; and even if they had been right, his conclusion would not follow from them. By proper mechanical appliances, such as hydraulic presses, we may make a given amount of heat raise almost any weight we please, whether it be applied to heat air, steam, or vapour of turpentine; but let us suppose that instead of "raise ten times the weight," it were written "do ten times the work." The specific heat of vapour of turpentine is just ten times that of air; so that we may conclude that Mr. Highton means that it will require ten times as much heat to expand a given volume of turpentine by a certain amount against a given pressure as would be required if the turpentine were replaced by air. This will be a little inaccurate, as the coefficient of expansion of a vapour is always greater than that of a permanent gas. Now, this heat thrown in has these effects: a portion, H_1, is used simply to heat the gas to raise its temperature; a second part, H_2, does internal molecular work, by separating the particles against their own cohesion; whilst a third, H_3, expands the gas against the external pressure, and produces the work which we measure in foot-pounds. It is easy to see that Mr. Highton compares these foot-pounds with the whole heat thrown into the gas, $H_1+H_2+H_3$, instead of the heat, H_3, used in producing them. In a gas, H_2 is very small, as the experiments of Joule and Thomson show; but in a vapour it is considerable. Mr. Highton's answer to the supposed objector is altogether irrelevant. The objector might have added that the heat also raises the temperature of the gas, and he does not assume that this is equivalent to work measured by foot-pounds, though it is only that some of the heat is used to effect the expansion and the heating, whereas Mr. Highton unwittingly assumes that none of the heat is so applied, but that all goes to lift the weight.

ELECTRICITY.

The Passage of Electrical Currents through Rarefied and other Atmospheres.—Mr. C. F. Varley, the electrician, has recently made several new discoveries in relation to the passage of electricity through rarefied gases, and through the atmosphere at its normal pressure. Some of these discoveries were made known in papers read before the Royal Society a few weeks ago, and several were shown at General Sabine's *soirée* at Burlington House a fortnight since. In his experiments he uses Geissler's vacuum tubes, which have been exhausted by chemical means to a degree ten or fifteen times as attenuated as the vacuum which can be produced by the best made air-pump. Such a tube shows stratifications in the light when an electrical current is passed through it. He has discovered that four distinct kinds of luminous discharge may be produced by graduating the strength of the current flowing through the tube, by introducing various amounts of resistance into the circuit. The results may be summed up as follows:—1. With an extremely feeble current of high intensity and small quantity, the luminous positive pole is alone visible. This light, although scarcely visible to the eye in the darkest room, photographs itself perfectly on a photographic film of wet sensitised collodion; an exposure of thirty minutes with a double-combination portrait lens is, however, necessary. 2. On diminishing the resistance so as to increase the quantity, a tongue of light projects from the positive towards the negative pole, and the two poles become luminous. 3. On still further augmenting the quantity, the negative pole only becomes luminous. 4. Lastly, on passing a momentary current of great quantity and intensity through the tube, the positive pole alone is luminous, but in spots only, and not all over as in the first case. All these experiments have been photographed by Mr. Varley.

A bell struck in a vacuum produces no noise. In the performance of the fourth of the foregoing experiments, the momentary discharge of electricity through the vacuum produces a sharp distinct "click" of a peculiar sound.

Mr. Varley has also discovered that four different kinds of discharge may be obtained through air, between the two conductors of a powerful Holtz's electrical machine, capable of giving sparks from eight to eleven inches in length. In the course of his experiments, he observed one kind of discharge, which seems to clear up the mystery hanging over that very rare phenomenon —ball lightning.

When a small strip of paper, pointed at both ends, is attached to the knob of the negative conductor of the Holtz's machine, the two ends of the paper being bent, so as to point towards the knob of the positive conductor, and when the two conductors, fully and continuously charged, are placed rather too far apart for the spark to pass, two bright spots are seen upon the negative conductor. The first impression with regard to these spots is that there are two particles of dust on the positive pole, acting as points to throw off electricity. That such is not the case is easily shown by rotating, first the negative, and then the positive pole. When the positive pole rotates, the luminous spots do not rotate with it; but when the negative pole rotates, and the positive is stationary, the two luminous spots on the positive pole rotate with the negative. If, therefore, a negatively charged cloud should be passing over the earth, and one corner of the cloud be nearer to the earth than the remainder, a luminous ball might possibly be seen running over the surface of the ground, and following the movements of the cloud above. When the cloud came sufficiently near the earth for the spark to pass, there would be a flash of lightning and a crash of thunder. Mr. Varley cautiously observes that this *may* explain the phenomenon known as ball lightning, which is too well attested to be doubted, but which, from the absence of explanation, has been hitherto unbelieved to be a reality by many men of science. This experiment was shown by Mr. Varley, for the first time in public, at the *soirée* previously alluded to.

On the same occasion, another interesting experiment was shown by Mr. Varley, in confirmation of an hypothesis which he mooted at one of the *soirées* last year, namely, that the stratifications in vacuum tubes are partially or wholly due to the presence of particles of matter detached from the negative pole. Plücker has shown that when a current of electricity is passed from the positive to the negative pole of a Geissler's tube, and the tube is placed over the poles of a very powerful magnet, the light is gathered up, and follows the course of the magnetic rays, no matter what be the direction in which the electric current is flowing. When the electric current and the magnetic rays run parallel to each other, a magnificent arch is produced, which extends on each side of the negative pole, beyond where there is any electric current flowing. Last year Mr. Varley stated, that from the nature of this phenomenon, he believed the arch to consist of incandescent particles detached from the negative pole, and controlled as to their course by the magnetic rays. The truth of this conjecture he has since supported by the following simple experiment:—A thin slip of talc, 1 inch in length, $\frac{1}{10}$th inch broad, and about $\frac{1}{10}$th grain in weight, is mounted on a single fibre of silk, and suspended inside the Geissler's tube. When this tube is placed over the poles of a powerful electro-magnet in such a position that the luminous arch shall not touch the talc, the passage of the electric current does not put the talc in motion. If the tube be so shifted that the arch plays upon the lower portion of the talc, it repels it. If it be still further shifted, so as to throw the arch against the upper portion of the talc, it is repelled, although in each case, from the position of the poles, there is no electric current whatever passing in that part of the tube which contains this delicate tell-tale. When the arch is allowed to play against the almost invisible fibre of silk which suspends the talc, the fibre is in no way injured. A still further confirmation of the truth of the hypothesis is, that that portion of the arc which strikes the talc produces a luminous cloud brighter than the rest of the arch, indicating that the matter is arrested and condensed there; the remaining portion of the arch is cut off by the intervention of the strip of talc.

The most singular thing, perhaps, in this experiment is the fact that this transfer or projection of matter from the negative pole, is in the opposite direction to that in which, *à priori*, it might be expected to pass. It is in the reverse direction to that in which particles of carbon are carried between the carbon points of the working electric lamp. Mr. W. R. Grove noticed, many years ago, when experimenting with the voltaic arc in air on a very large scale, that if the negative pole were of platinum, while the positive pole was carbon, there was a transfer of carbon from the positive pole to the negative; but, in addition to this, particles of platinum appeared on the carbon, they having been transferred from the negative pole to the positive, in obedience to some law at present unexplained.

METEOROLOGY.

The Meteorological Office has published Parts II. and III. of its "Quarterly Weather Report for 1869," containing the lithographed curves and explanatory chronicle for the six months from April to October.

We have also to welcome a new and revised edition of the "Board of Trade Barometer Manual," which has just appeared. This little book, which runs to the length of 70 pp., contains a good deal of new matter compared with preceding editions. Among other points, we may notice a chapter on "The Present Condition of our Weather Knowledge in Connection with Meteorological Telegraphy," by Mr. Scott; a chapter on "The Use of the Barometer to Seamen," by Captain Toynbee; and a short description of "The Most Usual Forms of the Barometer, with some Tables for Barometrical Reductions." The text is illustrated by a few woodcuts and some plates, and the whole pamphlet forms a much more complete and satisfactory manual than those formerly issued by the department.

We are glad to notice that since the beginning of the year the "Shipping and Mercantile Gazette" has commenced the issue of a daily wind chart for these islands, the information for which is supplied by the Meteorological Office. The chart is prepared on a plan devised by Captain Charles Chapman. The publication of these charts is a great step in advance, and we learn that they have been received with very general approval by the subscribers to the "Gazette."

The last number of the "Proceedings of the British Meteorological Society" contains a paper by Mr. Dines, "On Evaporation and Evaporation Gauges." The experiments seem to have been very carefully carried out; but as the author of the paper himself remarks, a much more thorough investigation of the subject is requisite before laws can be laid down. The mode of testing the evaporation, on a small scale, was to place a vessel of water on one scale of a delicate balance, and to counterpoise it exactly with weights. If then evaporation be taking place, the weight of the water will decrease; if, on the contrary, condensation be going on, the weight will increase. In either case, the rate at which the weight changes can be measured, and also the temperature at which condensation ceases and evaporation begins. Theoretically this temperature should be the dew-point as given by independent hygrometrical observations, and, practically, Mr. Dines found that in the saturated atmosphere of a greenhouse the two temperatures nearly agreed; but that in the ordinary atmosphere of a room the temperature at which the water on the scale of his balance ceased to increase in weight was sometimes as much as 3° or 4° below the dew-point, as given by the wet and dry bulb hygrometer. Mr. Dines's evaporating gauge is a large cistern, with a smaller cistern beside it, connected with it by a pipe at the bottom, so that hydrostatic equilibrium is always preserved. The level of the water in the small cistern is observed by means of a ball which floats on it, and is attached to an arm hinged at the bottom of the cistern. The arm may be prolonged beyond the ball, so as to describe a large arc and give an open scale. The remainder of the number is mainly taken up with accounts of the Auroras of October 24, 25, as observed by the several assistants at Greenwich Observatory. As might be expected, the individual observations correspond very well with each other, so that the notices might fairly have been condensed from 16 pages into three or four.

Mr. Blanford has published a paper "On the Normal Rainfall of Bengal," in the "Journal of the Asiatic Society." Dove's notice of this region, in his "Rain Tables for the Globe," contained in the first part of his "Klimatologische Beiträge," is confined to the figures for 12 stations. The number of stations included in Mr. Blandford's paper is 47, and the Presidency is divided into ten districts.

The principal results of the discussion are:—

1. The rainfall of Eastern begins earlier, and is on the whole heavier than that of Western Bengal, at stations equally distant from the sea, and at equal heights above its level.

2. The south-west monsoon of Eastern Bengal is probably induced by the rarefaction over Thibet; that of Western by rarefaction over the Punjaub.

3. Western Bengal receives, in addition to the normal rain of the south-west monsoon, a slight precipitation during the cold season, and also some irregular spring rains.

From the other side of the Atlantic we learn that in the United States a system of telegraphic weather reports and of storm warnings is in process of organisation, under the superintendence of General A. J. Megens, the chief signal officer. This is a most important measure, for hitherto the meteorological observations taken in the United States, though in themselves copious and valuable, have been of an isolated and, therefore, desultory character, as was unavoidable, owing to the vast extent of territory covered by the stations. It is, however, to be regretted that, in drawing up the report which announces the proposed scheme, care has not been taken to resort to the latest sources of information in Europe. The state of things described as existing in this country and in France in 1870 is that which prevailed in the years 1862-3. No notice whatever has been taken of the discontinuance of weather forecasts, or of the other changes which occurred on the death of Admiral Fitzroy.

While speaking of the development of meteorology in the States, we should not omit to notice that the government of Canada are also organising a general system of observation for the several provinces of the dominion. Among the proposals for storm-warnings in America, there is one by a Mr. Watson which is rather amusing. He suggests that notice of storms should be given by the discharge of artillery!

The Common Council of the City of New York have established a very complete meteorological observatory in the Central Park, under the management of Mr. Daniel Draper. This observatory is fitted with self-recording instruments, somewhat similar in principle to those at the observatories in connection with our own Meteorological Office. Experience will show whether the alterations introduced by Mr. Draper are improvements or the contrary. Among other instruments there is a self-registering rain-gauge. The description of the instruments will be found in the "Thirteenth Annual Report (1869) of the Committee of the Central Park."

In Holland, the Marine Department of the Meteorological Institute, after eight years of nearly total silence, has resumed the publication of results. The present issue, edited by Lieutenant Comelissen, consists of "Sailing Directions from Java to the Channel," and is a new edition of the work published by Lieutenant Audran in 1858. It is in two parts. Part I. is in the form of an atlas, and exhibits the actual crossings of each meridian and the length of voyage for each ship. The epoch chosen for classifying the passages is the month in which they cross the parallel of St. Helena, being about the middle of the voyage. Part II. contains a discussion of the results, with sailing directions based upon them. Of the two Appendices, the first is "On the Storms of the Southern Hemisphere;" the second consists of "Hydrographical Notices."

Herr von Freeden has brought out No. III. of the "Mittheilungen of the Norddeutsche Seewarte," which is a discussion of the tracks of the North German Lloyd's steamers from Bremen to New York—374 passages in all. Unfortunately, as the logs date from the period prior to the establishment of the Seewarte, they contain no barometrical or thermometrical observations, referring exclusively to wind and weather, distance made good every day, and amount of coals expended. The specially meteorological portion of the work consists of remarks on Ice, Storms, and Wind.

It appears that ice is principally met with between the meridians of 46° and 51°, and is rarer to the westward of that district than to the eastward.

The storms are analysed carefully, and it would be impossible for us to give even a satisfactory sketch of the discussion. The direction from which they most frequently blow is, on the whole, between W. and N.N.W. As regards the time of their occurrence, 50 per cent of the entire number are recorded during November, December, and January; 26 per cent during February, April, and October; 12 per cent during March and September; and the remainder are distributed over the four months from May to August. The examination of the storms with reference to longitude shows that they reach a maximum for 30° W., and maintain it to 45° W., their direction being northwesterly, *i.e.*, between W. and N.N.W. West of the meridian of 55° W., the majority of the storms are north-easterly. From this relation Herr von Freeden concludes that the storms have their origin not far from the Banks of Newfoundland, where the cold Arctic current meets the warm waters of the Gulf Stream, and that they are not West India hurricanes crossing the Atlantic from shore to shore.

The general winds of the North Atlantic for the ships' tracks are also discussed, and the results given in the form of a chart.

The author concludes by expressing a conviction, in which we thoroughly agree with him, that the cause of meteorology will be more benefitted by the careful investigation of observations taken over limited areas of the earth's surface than by all the theoretical disquisitions on general atmospherical circulation which are constantly emanating from the "depths of the self-consciousness" of German Professors, and are invariably so attractive to the non-scientific public.

We have received a number of Annual Reports from the Continental Meteorological Observatories. That from Saxony is for the year 1868. It contains complete monthly means, &c., from 25 stations, with five-day means of pressure and temperature. In addition there are a series of tables of earth temperature at various depths, and also of the depth of water in wells at Dresden, Leipzig, and Zwickau, and of the levels of the Elbe and Moldau, and the other rivers of Saxony. Professor Bruhses has also brought out the Report of his own Observatory at Leipzig for the year 1869. In Wurtemburg Dr. Schoder has printed a brief paper on the meteorological conditions of that kingdom, as determined by several years' observations, taken at 22 stations in connection with the Statistical Bureau. His Report for 1869 contains full tables for all the stations for the year, and a general notice of the special phenomena of the several months.

The fifth volume of the "Jahrbuch of the Central Austalt in Vienna," contains the usual tables of mean results, but instead of the daily deviations of the instrumental readings from their mean values, Dr. Jelinck has reprinted the daily telegraphic reports from 15 stations.

We are glad to learn that a New Meteorological Institute has been established in Hungary. Its head-quarters are at Pesth, under the directorship of Dr. G. Schenzl. It commenced work at the beginning of the present year.

Dr. A. von Dettingen has brought out the Third Report of the Observatory of Dorpat. It does not contain much that is new, excepting a method of traversing ordinary observations of wind as to direction and force. A dial is fitted to the indicator of the vane; and the different quadrants of the circle are divided so as to show the north (or south) and the east (or west) components at a glance. It is announced that the form of the Report will be altered in future, as Dorpat is to be the central observatory of one of the Departmental Meteorological Organisations of the empire, on the plan sketched out by Dr. Wild, (Quart. Journ. Science, vol. vii., p. 415).

Dr. Jelinck has published a short paper in the "Proceedings of the Vienna Academy," "On the Annual Distribution of Thunderstorms in the Austrian Empire." He only deals with the days on which electrical phenomena were recorded, and not with the individual storms. He finds that the storms are almost exclusively summer storms, excepting at the Adriatic stations, such as Trieste and Lesina, where, however, the winter storms form only a small percentage of the whole. This result shows the marked contrast which exists, as regards thunderstorms, between the conditions of Central and Southern Europe, and those of North Scotland, as stated by Mr. Buchan in his paper ("Journal of the Scottish Meteorological Society," vol. ii., p. 344).

Another paper, by Dr. Jelinck, in the same journal is on "The Annual Range of Temperature at Trieste, Klagenfurt, and Arvavakalja.

We have nine numbers of the "Journal of the Austrian Meteorological Society" to notice, but they do not contain many papers of much importance. Dr. Hann gives us two more papers on the climate of South America, in continuation of that noticed in No. 29. The first is on the "Southern Stations of Chili, Puerto Moult, Valdiera, and Conception;" the second on Central Chili, Santiago, and Valparaiso.

We have already spoken of the contrast, as regards climate, between the eastern and western extremities of the Straits of Magellan; differences quite as remarkable as these exist between the meteorological conditions of Patagonia and those of Central Chili. On the southern part of the coast the glaciers, even in 46½° S., come down to the sea, and the snow-line between 41° and 43° is at the level of 6000 feet above the sea. Ten degrees further to the north the snow-line rises to the level of 14,500 or 15,000 feet. Southern Chili and Patagonia are among the wettest districts of the globe, while Central Chili is rainless for seven months in the year, and its soil is at times a desert, unless where irrigated. The Isotherm of 50° cuts the west coast of South America in about the same latitude (south), as the corresponding Isotherm cuts the East Coast of Asia and America in the Northern Hemisphere. The summer is on the whole about 10° or 15° cooler than that of corresponding places at the other side of the equator, and the climate is, therefore, pre-eminently insular. For Central Chili Dr. Hann gives us observations for Santiago and Valparaiso, and bases his remarks chiefly on the accounts furnished by Gillis in the Report of the United States Exploring Expedition. The climate is, on the whole, subtropical, but excessively dry, excepting during the rainy season. Thunderstorms are very rare and excite no less astonishment than earthquakes. The range of temperature is moderate, much more so than in corresponding latitudes in the Northern Hemisphere. The average temperature of Santiago is 13° lower than that of Beirat. The low summer temperature on the coast is due to the proximity of the cold water of the Humboldt current.

Another paper is on "The Climate of Mesopotamia," based principally on the observations of Dr. Schläfli and Lieut. Collingwood. This region is one of the hottest in the world, as although it is in latitude 35° N. the Isotherm of 95° passes over it in July. In summer, life in Bagdad is entirely regulated by the temperature, for from 10 a.m. to 5 p.m. the heat above ground is unendurable, and the whole population descends into cellars called "surdabs." On emerging from these in the evening the rooms and bazaars are still too hot to be habitable, having been exposed to the sun all day long; supper is accordingly taken on the flat roofs of the houses, where everyone sleeps, in the open air, without fear of injury, owing to the non-existence of dew.

Raulin's paper on "The Rainfall of Algeria," which appeared in the "Comptes Rendus," is also reproduced in abstract.

Dr. Hann gives a notice of the remarkable phenomenon of increase of temperature with the height, so well known in the Alps. He traces out the actual fact very carefully, but does not attempt to assign a cause for it.

Dr. Wojeikoff gives a most interesting note on "The Ice and the Water Level of the Volga, in Relation to the Clearing of the Country," based on the observations taken at Astrachan, for the years 1830-67. The following table shows the means for the four nearly equal divisions of that period:—

	Froze.	Thawed.	Flood began.	Highest level.	Flood ended.	Greatest depth of water.	Difference in days.			
	(*a*)	(*b*)	(*c*)	(*d*)	(*f*)	Eng. ins.	*a-b*	*b-c*	*b-d*	*c-f*
1830-37	Dec. 18	Mar. 22	Ap. 30	June 16	Aug. 17	104	94	39	86	109
1838-47	,, 14	,, 23	,, 28	,, 18	,, 31	90	99	36	87	125
1848-57	,, 22	,, 26	,, 29	,, 14	Sept. 12	114	95	33	79	136
1858-67	,, 10	,, 29	,, 22	,, 13	Oct. 31	118	108	24	76	192
Average	16	25	26	15	Sept. 16	106	99	30	78	143

The duration of the ice has somewhat increased, the river freezing earlier and thawing later, but these changes are immaterial. The alteration in the time, extent, and, above all, duration of the floods, is most striking. The fact that the flood comes down eight days earlier is attributable to the great clearing of the forests on the banks of the Volga itself, which has the effect of allowing the snow to thaw sooner. The highest flood is noticed when the northern tributaries yield their quota; and as in their basins no clearing of consequence has as yet taken place, their floods have not become much earlier in date, and the highest level is only reached at Astrachan three days earlier than was formerly the case. The duration of the floods, which in the last 10 years is nearly double what it was at first, is probably to be accounted for by the fact that clearing of woods promotes more rapid drainage, and so the floods in the rivers and the desiccation of the soil proceed *pari passu.* The continuance of the flood is therefore due, not to the melting of the snow, but to the rainfall, which was formerly retained on the land and is now discharged more rapidly.

In connection with these changes it is very interesting to learn that the level of the Caspian has risen very materially of late years, as is proved by the soundings in the port of Baku. M. Wojeikoff promises to return to the subject at a future date.

The journal also contains full descriptions of the auroras of October 24 and 25, from several European Stations.

MINERALOGY.

Although it is only within the last few years that attention has been forcibly directed to the importance of studying the minute structure of minerals and rocks, sufficient work has nevertheless been already accomplished to encourage the belief that the microscope will eventually become almost as important an aid to the mineralogist and petrographer as it has long been to their biological brethren. Among the most successful workers in the department of micro-minerology may be mentioned Professor Zirkel, of Kiel, whose recently published researches* it becomes our duty to notice.

Whilst it is well known that crystals of quartz frequently contain many microscopic cavities charged with certain fluids, such enclosures are but rarely found in crystals of felspar. It is, therefore, not without interest that Zirkel announces the discovery of a very large number of fluid cavities in the felspar of a coarsely crystalline rock from the Isle of Mull, consisting of plagioclase, diallage, and olivine. It is found, too, that the crystals of labrador-felspar from the olivine-gabbro of the Isle of Skye are rich in similar fluid cavities. In many of the liquid-bearing hollows with which quartz is frequently charged, may be detected minute cubic crystals, usually clear and smooth faced, but sometimes having their planes striated in squares. Zirkel finds that many of these cubes consist of sodium-chloride, and believes that the fluid which encloses the crystals is a solution of the same salt—results which corroborate the conclusions advanced many years ago by Mr. H. C. Sorby.

Attention is directed by the same author to the wide distribution of microscopic crystals of apatite, and a long list is published of many eruptive rocks—diorites, melaphyres, and diabase—in which this mineral may be detected. The author also describes some curious crystals of leucite which enclose foreign particles disposed in a concentric radiate form distinct from the zonal arrangement which often obtains in this mineral. The microscopic structure of elæolite from the zircon-syenite of Laurovig and Frederiksvärn, in Norway, has likewise been studied by the author, who shows that the mineral from these localities is never a pure substance, but consists of a colourless mass in which are disseminated microscopic crystals of hornblende, to which the green colour of the elæolite may be referred. But perhaps the most interesting of Zirkel's results is the discovery of microscopic tridymite—Vom Rath's new species of silica—widely distributed through trachitic rocks. In the rock of San Cristobat, near Pachuca, in Mexico, the mineral occurs in colourless six-sided plates, having a length and breadth rarely exceeding 0·02 m.m., the outlines being somewhat rounded, and the crystals grouped together in a way which seems to be characteristic of this species. Tridymite is also found in crystals, both microscopic and macroscopic, in the sanidine-trachyte of the Drachenfels and Perlenhardt in the Siebengebirge on the Rhine.

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*Leonhard und Bronn's Jahrbuch für Mineralogie, U.S.W, Heft 7, 1870, p. 801.

Zirkel shows that the small rounded granules of serpentine so widely disseminated through certain crystalline limestones have in many cases resulted from the alteration of olivine, and it seems likely that his researches in this direction may eventually shed some light on the origin of the so-called eozoonal structure exhibited by many serpentinous limestones.

We are glad to learn that one of our English chemists—Mr. John Arthur Phillips—is busying himself with the chemical and microscopic study of some of our British rocks and minerals.[*] Many of the old ore-bearing rocks of Cornwall have received Mr. Phillips's attention; but so far as they are rock-masses and not minerals, any notice of them scarcely falls within the scope of this chronicle. One of the subjects of our author's study is, however, a felspathic mineral which is quarried at the glass mine, near Roche, and exported to the potteries. It is a yellowish white crystalline mineral, enclosed in a schorlaceous granite, and traversed by veins of milky quartz. On analysis it was found to be an orthoclase containing 10·37 per cent of potash, and 2·4 of soda, with about 1·6 of lime; whilst its monoclinic form was determined by Professor Miller's crystallographic measurements. Analyses are also published of the beautiful serpentine of the Lizard—a substance which may be regarded with equal propriety as a mineral or as a rock. The specimen analysed by Phillips presented a dark green colour, thickly spotted with red, and exhibited under the microscope a crypto-crystalline base, with spots of oxide of iron, accompanied by indistinct crystals of a green or yellowish brown colour, which it is suggested may be pseudomorphs after pyroxene.

An interesting essay on the relation subsisting between chemistry and mineralogy has been published by Professor Rammelsberg.[†] At once a distinguished chemist and an excellent crystallographer himself, he maintains that every mineralogist should be equally familiar with both these sciences, and points out the mischief which must necessarily result from any attempt at divorcing two branches of science so closely cognate as chemistry and mineralogy proper. Indeed, our author is bold enough to repeat the question which had previously been mooted, "Might it be allowed to call chemistry and mineralogy one science?" Believing that it is the duty of all who are interested in mineralogy to keep pace with the progress of modern chemistry, he denounces the persistence with which many mineralogists cling to the old-fashioned atomic weights and rational formulæ. In these respects Rammelsberg's views are directly opposed to those of Von Kobell, or at least to those which he held a year or two ago.[‡] We believe, however, that any-one who follows up mineralogical literature cannot fail to observe that the conservatism which has long been imputed to the mineralogist is gradually yielding, and that at any rate most of the younger men cannot be accused of lagging behind their age.

At the request of Professor Weisbach of Freiberg, Herr Frenzel has analysed the two minerals which were recognised as distinct many years ago by Breithaupt under the names of *Plumbostib* and *Embrithite*.[||] These new analyses show that the two minerals in question have identically the same chemical composition, both being represented by the formula $10PbS+3SbS_3$. It is probable that they should be consolidated into one species; and if this be done, our author suggests that of the two names in vogue "Plumbostib" should fall to the ground.

O'Reileyite is the name which Mr. D. Waldie proposes[§] for a new Burmese mineral obtained from the late Mr. O'Reiley, of Martaban. It contains arsenic, iron, and copper, approximately in the proportions of two equivalents of As, six of Fe, and one of Cu.

Mr. J. S. Adam has analysed some specimens of the rare mineral *Gahnite*, or zinc-spinel, found by Professor Brush, at Mine Hill, Franklin Furnace, New

[*] Philosophical Magazine, Feb., 1871, p. 87.

[†] Chemical News, Feb. 10, 1871, p. 64.

[‡] See Quart. Journ. Science, July, 1868, p. 416.

[||] Journ. für prakt. Chemie, No. 18, 1870, p. 360.

[§] Chemical News, Jan. 6, 1871, p. 4.

Jersey.* In affecting a cubic instead of an octahedral form the specimens are crystallographically unique, whilst they are notable chemically for the large proportion of zinc which they contain—40 per cent of zinc oxide, and 50 of alumina being the main constituents.

An analysis of the Stewart-county meteorite has been published by Mr. J. Lawrence Smith.† The stone fell at about 11.30 a.m., on October 6th, 1869, in Stewart Co., Georgia, U.S.; and the details of observations relating to its fall have been collected by Professor Willet. A single stone has been found weighing 12½ ozs., and covered externally with a dull black coating. It presents an irregular conical shape, has a specific gravity of 3·65, and contains 7 per cent of nickeliferous iron, 6·1 of magnetic pyrites, and 86·9 of bronzite, olivine, albite or oligoclase, and chromic iron.

It has been shown by Professor Church that many of the hyacinth-coloured gems usually known to collectors and jewellers as *jacinths* or *hyacinths* are nothing more than garnets of the variety termed *essonite*. Indeed, the true red zircon, or hyacinth proper, is much rarer than has generally been imagined, and hence it is interesting to learn from Professor Church‡ that certain rolled pebbles lately brought to this country from Mudgee, in New South Wales, are veritable hyacinths, and agree in their pyrognostic characters with the well-known zircons of Expailly, in France.

About three years ago attention was directed to the occurrence of gold on the Cudgegong River, in New South Wales, and a great rush was accordingly made to a locality known as the Two-mile Flat. It was not long, however, before diamonds were detected by the gold-diggers; and although the discovery attracted at first but little notice, workings in quest of the precious gem were eventually set on foot by the Australian Diamond Mining Company. A visit to the washings on the Cudgegong, near Mudgee, has enabled Mr. Norman Taylor and Professor A. M. Thomson, of Sydney, to prepare some interesting notes on the occurrence of the diamond in this locality.

The diamonds are distributed somewhat sparingly and irregularly through certain outlying patches of an ancient river-deposit, similar to what is known in Victoria as the older Pliocene drifts.

They are for the most part transparent and colourless, but many present a pale straw tint; whilst some have been found of green, brown, and even black colours. One opaque black diamond has been recorded. The average size of the stones is small, and the largest hitherto discovered weighs only 5⅝ carats. Many of the diamonds are well crystallised, and it is notable that their faces and edges have suffered little or no abrasion, whilst the accompanying minerals have been much water-worn. The most characteristic of these minerals are a vesicular black pleonaste, or spinel; brookite, or oxide of titanium, mostly in flat reddish transparent plates; and a bluish-white opaque corundum in six-sided slightly barrel-shaped prisms, with flat terminal planes. In addition, however, to these minerals, which are especially characteristic of the diamond-bearing deposits of the Cudgegong, there are found also topazes, generally white, in water-worn fragments and sometimes in crystals; quartz in ordinary double-hexagonal pyramids, and in pebbles including varieties of cornelian, jasper, and agate; sapphire—blue, green, yellow, or parti-coloured; ruby occurring but sparingly, and in small flat grains; Barklyite, or an opaque magenta-coloured corundum; zircons in brown, red, and colourless fragments, and also as a sand; tourmaline in rolled black prisms; black magnetic and titaniferous iron-sand; garnets; wood-tin; and native gold. The authors enhance the value of their paper by publishing analyses of the pleonaste and of two of the varieties of corundum which occur in association with the Cudgegong diamonds.

Those who are interested in the diamonds of South Africa, and the geological conditions under which they are found, may consult with advantage an excellent paper on this subject recently published by Professor T. Rupert Jones.*

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* Silliman's American Journal, Jan. 1871, p. 28.
† *Ibid*, Nov., 1870, p. 339.
‡ Chemical News, Feb. 17, 1871, p. 78.

MINING.

A new self-extinguishing safety-lamp has been devised by Mr. W. Simpson, of Battersea. It is constructed for burning paraffin oil, so that the collier may be furnished with a more brilliant light than usual, and therefore be less disposed to remove the wire cage. Any attempt to do this, however, will immediately bring into play the automatic extinguisher. This consists simply of two small metal plates, which are caused to fall upon the wick and wick-holder by means of a spring that is released on unscrewing the cage from the body of the lamp.

Attention has also been lately directed to the magnetic lock applied to the safety-lamp some short time ago by Mr. S. P. Bidder, jun. In this lamp the bolt of the lock can only be withdrawn by the action of a powerful magnet, and a strong electro-magnet is, therefore, permanently fixed in the lamp cabin. As an instrument of the requisite strength could not be carried about, it appears that the illegitimate opening of the lamp is placed beyond the power of the working collier.

A simple form of safety apparatus for preventing accidents in shafts from breakage of the ropes or from over-winding, has been invented by Messrs. Turner, Grey, and Brydon, of Barrow-in-Furness. As in many other forms of safety machinery, the cage is sustained in its descent by the action of chisel-pointed pawls which are caused to clutch the wooden guides. These catches are connected by means of levers with a bar which passes through a vertical cylinder fitted with india-rubber springs, and is attached at its other end to the rope. In the ordinary action of the cage the springs are retained in a state of contraction, and the clutches glide freely up and down quite clear of the guide-rods; but on rupture of the rope the springs expand and the clutches are brought into play. In favour of this apparatus, it may be said that the safety arrangement comes into action every time the skip completes its journey up or down the shaft; and being, therefore, in constant use, there is no fear of its being out of order at the fatal moment when an accident occurs.

To obviate the inconvenience of having the apparatus which develops the motive power connected directly with the cutting-tool, Mr. George Simpson has invented coal-cutting machinery by which the moving power may be long retained in one position, whilst the tool alone moves with the advance of the work. Different forms of cutter for excavating the fuel are employed; but it would be difficult to describe the mechanical details without the aid of diagrams. They may all, however, be brought into relation with the apparatus for developing motive force—which may be steam, compressed air, or manual labour—by means of pulleys and belts, or other modes of transmitting motion.

METALLURGY.

It is matter of familiar observation that certain metals—especially iron and steel—are more readily broken in cold than in warm weather. The inconvenience arising from this apparent increase in brittleness is especially felt by the railway engineer, who well knows that more accidents occur from fracture of rails, tyres, and axles in winter than in summer, or in a severe climate than in a mild one. But while none will probably deny this fact, opinion is strangely divided as to its explanation. On the one hand it is affirmed that the tenacity of the metal is actually lowered by a reduction of temperature; and this seems to be the common opinion entertained by practical men, who constantly speak of iron becoming brittle when "the frost is in it." On the other hand, there are not wanting many eminent men of science who maintain that the strength of the metal is not reduced by lowering the temperature; it is true, they admit, that a rail, for example, is broken more readily during frost, but this is not because the metal is really more brittle, but simply because the road on which the rail rests becomes rigidly frozen, so that the permanent way loses its elasticity, and becomes subject to a series of sharp concussions every time the rolling load passes over it. This subject is one not only of paramount interest to the metallurgist and engineer, but also deeply concerns the public at large. It is not, therefore, surprising that the severity of the past winter, and the consequent accidents, should have led to the discussion of the question anew. A batch of papers has indeed been presented to the Manchester Literary and Scientific Society, an abstract of which it is our duty to lay before our readers.

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* Geological Magazine, Feb., 1871.

Mr. W. Brockbank contributed some "Notes on the Effect of Cold upon the Strength of Iron." Some of these experiments relating to the resistance of cast-iron to a transverse strain were performed at the beginning of the year at the works of Messrs. Jackson and Co., of Salford. Good castings were obtained by the judicious admixture of several suitable forms of pig-iron, and the experiments were thrice repeated in order to obtain fair results. These results show that bars of cast-iron suffered a considerable diminution of strength and elasticity when the temperature was reduced below the freezing point.

A paper "On the Properties of Iron and Steel as Applied to the Rolling Stock of Railways," by Sir W. Fairbairn, was read at the same meeting, and took an opposite view of the subject. The author believes that temperature has little or nothing to do with the strength of the metal. He finds, for instance, that the tensile strength of wrought-iron plates is as great at zero as at a low red heat; indeed, the breaking-weight was slightly greater at the lower temperature. At a red heat, however, one-half of the strength was lost.

Some notes "On the Alleged Action of Cold in Rendering Iron and Steel Brittle," were communicated by Dr. Joule. His experiments were made upon a dozen darning needles, one-half of this number being tested in a freezing mixture, and the other half at the ordinary temperature. The test was applied by attaching a wire to the middle of the needle and pulling it by a spring weighing machine. The breaking-strain was found to be rather greater in the cold than in the warm needles, and Dr. Joule believes that accidents arise mainly from badly-selected metal, and not from the reduction of temperature. Some experiments were also performed on cast-iron garden-nails cooled in a refrigerating mixture.

Another paper on the same subject, by Mr. Peter Spence, details some experiments on certain cast-iron bars which were subjected to transverse strain when cooled artificially. These experiments tend to show that reduction of temperature actually increases, *cœteris paribus*, the strength of cast-iron.

Reviewing all these experiments from a metallurgical stand-point, it is to be regretted that no chemical examination of the metal accompanied the mechanical tests. Everyone who knows the effect exerted on the physical properties of iron and steel by the presence of certain elements, such as phosphorus and sulphur, will feel that no general conclusion should be deduced from such experiments unless the chemical composition of the metal under trial has been determined.

We cordially welcome the appearance of a new technological periodical in the shape of "The Journal of the Iron and Steel Institute." The first number contains the proceedings of the Merthyr Tydvil meeting, held last September, under the presidency of the Duke of Devonshire.* His Grace opens the "Journal" by an introduction, in which he explains the object and scope of the periodical. It will, in short, contain a complete record of the progress made from time to time in the manufacture of iron and steel. In addition to the "Proceedings of the Institute," the number before us contains, among other original papers, the first part of an elaborate essay on the "Chemical Phenomena of Iron Smelting," by Mr. I. Lowthian Bell—an essay which gives the results of an experimental and practical examination of the circumstances which determine the capacity of the blast furnace, the temperature of the air, and the proper condition of the materials to be operated upon. A forerunner of this paper appeared in the shape of a lecture delivered before the Chemical Society some two years ago. One of the most valuable features of the new journal is the "Quarterly Report on the Iron and Steel Industries of Foreign Countries," prepared by the Foreign Secretary, Mr. David Forbes, F.R.S. In addition to Mr. Forbes's able report, some notes on the British iron and steel trades are contributed by the general secretary, Mr. J. Jones.

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* See Quart. Journ. Science, Oct., 1870, p. 561.

Mr. George Fowler, a mining engineer of Batsford, purposes to effect an improvement in the smelting of iron ores by reversing the conditions which obtain in the blast furnace. It need hardly be said that in the ordinary process the air which is forced in through the tuyeres near the bottom of the furnace forms carbonic acid (carbon dioxide) by the union of its oxygen with the carbon of the fuel; but that this gas in its ascent through the incandescent mass rapidly becomes reduced to the condition of carbonic oxide (carbon monoxide), which then deoxidises the ore, and is, in fact, the true reducing agent in the furnace. Inverting this action, Mr. Fowler injects a current of carbonic oxide at the top of the furnace, which, in its descent, permeates the ore and effects its reduction. He generates the carbonic oxide in a separate furnace, by the passage of a limited amount of air over ignited fuel, and then forces this heated gas into the furnace near the top, whence it descends through the mass of iron-ore, which it deoxidises, whilst it becomes itself partially converted into carbonic anhydride. That portion of the carbonic oxide which remains unaltered suffers combustion by contact with the blast from the tuyeres, and the heat thus generated suffices to fuse the metal and slag, which, therefore, fall into the hearth in a molten state. The heated carbonic acid and nitrogen are then passed through a Siemens's regenerator, where they are made to give up their heat before finally escaping.

The analysis of a genuine specimen of Wootz steel—the celebrated steel of native Indian make—by Professor Rammelsberg, has shown that it contains 0·867 per cent of carbon, 0·136 of silicon, 0·009 of phosphorus, and 0·002 of sulphur, in addition to iron. It may be remembered that in Faraday's analysis of wootz, a small quantity of aluminium was detected, and it is to the presence of this element that the excellent quality of the steel has often been attributed. Rammelsberg, however, has failed to find aluminium, thus corroborating the results obtained by Karsten and Henry.

A new mode of treating copper-pyrites, so as to form a rich matt previously to smelting, has been devised by M. C. M. Tessie du Motay. After fusion in a cupola, the pyrites is run into a peculiar roasting apparatus, which consists of two chambers or vessels connected below by means of a wide tubular channel. Air or oxygen is injected through a number of orifices into the fused sulphides, by means of which the roasting is effected; whilst by varying the pressure in the two chambers the material is forced alternately from one to the other, and is thus subjected to a mechanical stirring, which greatly facilitates the reaction. By repeatedly fusing and re-roasting the product, if needful, with the addition of silica, a rich matt is finally obtained, and this is then readily reduced and refined.

MILITARY, CIVIL, AND MECHANICAL ENGINEERING.

The defensive works carried out for the protection of Paris occupied by no means an unimportant part of our notes on engineering which appeared in the last number of this journal. The capitulation of that city in January last prevented any opportunity of fairly testing the success of such works, but the lessons to be learned from this war between France and Germany have been the means of causing other nations—and ourselves amongst the rest—to direct their attentions to the attainment of weapons of larger calibre, superior efficiency, and in far greater numbers than had heretofore been considered necessary. We purpose, therefore, to give a brief account of some of the principal arms now in course of adoption.

Rifles.—From a description by a military correspondent of "The Times," on the subject of French and German small arms, it appears that the chassepot is of a longer range, inflicts a more dangerous wound, and has proved a more effective weapon in every respect than the Prussian needle gun. The new

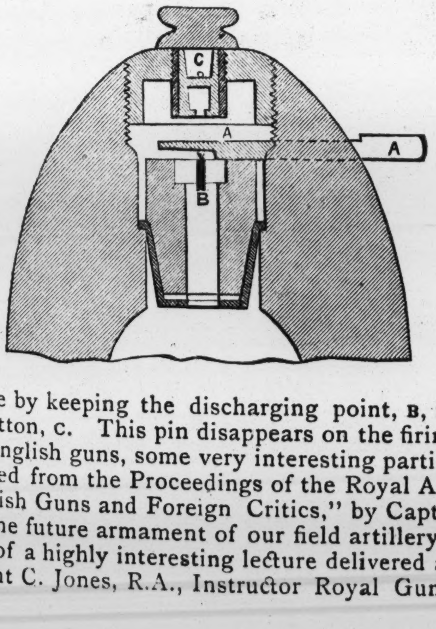
Bavarian rifle, the Werder, is better than either the chassepot or the needle gun; and its action as a breech loader is thought to be quicker even than that of the Martini rifle. It is a small bore, and the barrel has a sharply twisted rifling. With regard to our own army, the special committee appointed some time back to report upon the subject of small arms, has recommended the introduction of the Martini-Henry rifle throughout the service. In 1869, 200 of these rifles were distributed to the army for trial, with a view to their being subjected to exhaustive tests as to their suitability for hard usage, and the results appear to have fully borne out the recommendations of the Committee.

Artillery.—The principal French Artillery consists of breech-loading guns of 16, 19, and 24 centimetres. These guns are of cast-iron, strengthened by hoops, the trunnion ring being one of them. The vent is bushed with steel lined with copper. The rear end of the chase is closed by a screwed breech piece and gas ring. The dimensions, weight, and charges of projectiles for these guns are as follows:—

Projectiles for guns of	16 c.m.	19 c.m.	24 c.m.
Mean calibre (millimetres)	162·3	191·5	237·00
Weight of elongated shell (loaded)	31·5 k.	53·0 k.	100·00 k.
Weight of solid shot (steel or cast-iron) ..	45·0 k.	75·0 k.	144·00 k.
Charge for firing elongated shell	5·0 k.	8·0 k.	16·00 k.
Charge for firing solid shot	7·5 k.	12·5 k.	20·24 k.

Some of the shells fired by the Prussians into Paris during the bombardment measured 223 millimetres, or very little under nine inches in diameter at the base; were about twenty-two inches long, and weighed 88 lbs. Their percussion fuses failed to explode upon falling on soft ground; but they were causes of serious accident in more than one instance when carelessly handled, and the Government accordingly issued instructions regarding their treatment, in connection with which the following sketch was given of the arrangement of the percussion fuse, a copy of which appeared in "The Engineer" of 13th January last: "The arrangement will be seen almost at a glance, but it must be mentioned that the hole at the bottom of the fuze is covered with a piece of muslin kept in its place by a flat ring or washer of brass. The pin or plug, A, prevents accidental discharge by keeping the discharging point, B, from touching the fulminant in the button, C. This pin disappears on the firing of the gun."

FIG. 22.



With regard to English guns, some very interesting particulars are given in a pamphlet reprinted from the Proceedings of the Royal Artillery Institution, and entitled "English Guns and Foreign Critics," by Captain Vivian Dering Majendie, R.A. The future armament of our field artillery has also recently formed the subject of a highly interesting lecture delivered at the same institution by Lieutenant C. Jones, R.A., Instructor Royal Gun Factories, Royal

Arsenal, from which we have extracted the following particulars. It appears that the controversy of muzzle *versus* breech-loading guns has been practically determined in favour of the former. Bronze has proved too soft, and steel by itself too brittle, being liable to burst at any moment without warning, whilst wrought-iron alone does not possess the necessary hardness for the interior of the barrel, but a steel barrel strengthened with wrought-iron coils furnishes the very best construction yet discovered. Experience has shown that every campaign from the Peninsula War down to the Crimea has ended in a heavier projectile being adopted than was used at its commencement. The war on the Continent has proved this to have been true at the present day; and accordingly a new gun has recently been adopted for our field artillery. It is a 16-pounder shell gun, having a bore of 3·6 in., and a powder charge of 3 lbs.; it is built upon the Fraser principle of steel and iron, being rifled with three grooves, and its weight is 13 cwts. At the same time that a new pattern of field gun is being introduced, a new 35 ton gun for the navy has been manufactured and proved with 130 lbs. of powder. Twelve more of the same calibre have been ordered to be at once put in hand.

Some months ago experiments were conducted with different patterns of mitrailleurs against field guns with a view to ascertain their efficiency, and as to which was the best adapted for introduction into the British army. The results of these experiments were that the Gatling gun fired 492 lbs. of ammunition and scored 2803 hits; the Fosberry mitrailleur with 472 lbs. scored 1708 hits; the 12-pounder gun with 1232·5 lbs. scored 2286 hits; and the 9-pounder gun with 1013 lbs. scored 2207 hits. The whole series of trials form the subject of an elaborate report which has been made by the committee to the War Office. Twelve more of the Gatling guns have since been ordered to be made and issued to the troops for trial, in order to confirm the experiments which have already been carried out at Shoeburyness.

Torpedoes.—Our military authorities have decided upon forthwith preparing a large number of torpedoes, and Messrs. Spencelayh and Archer, iron-founders and engineers, of Chatham, have received orders from the War Office to prepare the cases for nearly 1500 of such weapons. On 7th December last Mr. Robert Weir, formerly of the United States Navy, read a paper before the New York Society of Practical Engineering on "Submarine Torpedoes." After giving some notice of what has already been accomplished in the way of submarine warfare, Mr. Weir proceeded to explain his device, which is in reality a submarine rocket, a slow burning composition being used as the propelling power; its whole construction and action is plain and simple, being made up of but two parts, the torpedo or exploding shell, and the rocket or propelling shaft. It is proposed to discharge them from guns or guides of simple construction secured in the vessel's sides or bows, from 8 to 10 feet, or even more, below the water line. They are made as nearly as possible of the same specific gravity as water, and are discharged by a friction primer. The best torpedo yet introduced into this country is one designed by Captain Harvey, R.N. This torpedo is proposed to be towed against an enemy's vessel, the torpedo ship, from which its movements are controlled, being a small quick-speed craft, from whose line of progression the torpedo has a divergence of 45°, which is due to the vertical plane of the torpedo being thrown at that angle by the manner in which it is slung. The casing of the torpedo is made of stout timber strengthened with iron straps at the ends and sides. The charge is, of course, dependent upon the size of the casing, and it is fitted with top and side firing levers, either of which, when pressed, acts upon another lever which presses down the exploding bolt, and thus fires the charge. It can also be adapted for firing by electricity, a special circuit closing apparatus having been designed for that purpose by Captain Harvey.

A committee has recently been appointed in this country with Captain Beaumont, R.E., M.P., as president, and Lieutenant Grove, R.E., and Mr. Abel, F.R.S., as members, to carry out experiments on the utilisation of balloons for reconnoitring purposes.

Tunnels.—On the 24th of last December the boring of the Mont Cenis

Tunnel was so far completed that the miners from the French and Italian sides of the mountain met in their respective underground passages. The advanced heading has been now effectually completed, and it remains but to complete it to the full necessary section in order to prepare it for the laying of the railway. A provision in the original contract established the obligation of the contractor to complete the contract by September next; but now, should the premium demanded by the contractor be granted, Signor Galery will undertake to complete the work by March, which would permit the tunnel line to be opened to Modena by the end of April. The tunnel is 12,200 metres, or about 7 miles 1020 yards in length. Boring operations were systematically commenced at both ends on 25th January, 1863, and the work will thus have occupied about seven years and two months. The cost of the tunnel was originally estimated at £2,600,000, but it is now believed that the outlay will not exceed £2,400,000.

Ship Canals.—The Portage Lake and Lake Superior Ship Canal has been cut through, and it is announced that it will be ready for the passage of the largest steamers on the northern lakes in the course of the present spring. This canal passes along the base of the promontory of Keweenaw, which is situated on the southern shore of Lake Superior, about midway between the eastern and western extremities, and extends in a north-easterly direction nearly one-third of the way across the lake. At the lower part of the promontory is a bayon called Portage Lake, which extends from Keweenaw Bay on the east nearly across to the west side. To complete the navigation, a canal two or three miles long had to be cut through a sand-ridge, and this part of the work has been accomplished at a heavy expense. It is probable, however, that the navigation of Portage Lake will have to be improved, and the channel deepened, before the largest lake steamers can pass through. The new route, it is stated, will shorten by 100 miles the length of the voyage between the Sault Ste. Marie Canal and the towns on the western coast of Lake Superior.

Railways.—The principal improvements of the present day with regard to permanent way, consist in the introduction of iron sleepers in the place of wooden ones. One great objection to cast-iron sleepers has always been their uncertainty to preserve the gauge. In order to obviate such defects, an iron permanent way has been designed by Mr. J. Cockburn-Muir, of Westminster. The sleepers in this system have been specially desined for the Vignoles rail, to which no altogether satisfactory application of a cast-iron support has hitherto been made. The rail is held without bolts or nuts, so that it is not weakened by punching. The form of the sleeper in plan is a parallelogram, to insure the largest amount of bearing surface, while the corners are rounded sufficiently to insure free flow of the metal in casting. The rail is held in position on the inner side by two lips cast on the sleeper, and on the outer side by a gripping-piece dropped in between the jaw on the sleeper and the foot of the rail, the gripping piece, or clip, being brought home on the rail by a taper-key of peculiar form, which is driven between the jaw and the back of the clip. The transverse bar is of T-iron, the dimensions of which are regulated according to the gauge and the weight of the engines to be used.

With the view of obviating railway accidents, Mr. William Naylor has recently introduced a continuous railway brake, which may be briefly described as an adaptation of the ordinary lever waggon brake to the purpose of working sets of brakes on a train of carriages. The levers which actuate the brake blocks are connected by other short levers to a spiral spring, which is kept in tension by means of a chain running under the entire train. When this tension is released, which can be done at pleasure, the spring presses the brake-blocks, through the action of the levers, against the wheels. Several arrangements have been designed by Mr. Naylor for tightening the chain, but want of space prevents further reference to the subject upon the present occasion.

A continuous railway brake, designed by Mr. E. D. Barker, of Weston-super-Mare, is, we understand, about to be tried upon the Great Eastern Railway. Mr. Barker employs hydraulic power to apply the brake-blocks to the wheels, and according to his plans each carriage has a main water-pipe running its entire length, and which is provided with branches leading to small hydraulic cylinders, a length of india-rubber tubing being used to connect each of these cylinders with the corresponding branch pipe. There is an hydraulic cylinder to each wheel, to which brake-blocks are applied, the ram with which the cylinder is fitted being connected to the brake-block nearest to it, while the cylinder itself is coupled, by a pair of rods, to the brake-block on the opposite side of the wheel. The cylinder is supported by the brake-hangers, and the water under pressure being admitted to the cylinder at the end furthest from the wheel, acts upon that end and upon the ram, thus forcing the brake-blocks into contact with the opposite sides of the wheels. This arrangement ensures uniformity of pressure on the blocks, and the thrust of one block being resisted by that of the other, no strain is thrown upon the axle.

GEOLOGY AND PALÆONTOLOGY.

Eozoön Canadense.—The genuine organic character of this oldest known fossil having recently been attacked, has brought a *resumé* of the grounds for so considering it from Dr. Carpenter and Professor Dawson, of Montreal. To the objection that it has hitherto only been found in metamorphic and structurally altered rocks, in which it would scarcely be possible for an organic structure to be preserved, Dr. Carpenter replies that the eozoönal structure is most characteristically displayed in those portions of the serpentine limestone of the Laurentian formation which have undergone the least metamorphic change. Principal Dawson also states that several other *Foraminiferal* forms allied to *Eozoön* which have recently been discovered in the Laurentian rocks of Canada, will shortly be described.

Duration of the Cretaceous Epoch.—Professor Wyville Thompson has recently propounded the theory that "we may be said to be still living in the cretaceous epoch," a view to which Dr. Carpenter has also lent his support. The arguments in favour of this theory rest, not only on the deposition over a large part of the North Atlantic sea-bed, at the present time, of a sediment closely corresponding with the chalk, and of the occurrence in it of a few types of life like the *Lingulæ* and *Terebratulidæ* belongingto the older formation; but by the pesistence of those which constitute the formation itself, viz., the *Globigerinæ*, the coccoliths, and the coccospheres; as also of numerous types of *Echinodermata* that were formerly considered essentially cretaceous, and of a great variety of those sponges (including *Zanthidia*) and *Foraminifera*, whose abundance in the white chalk is one of its most important features.

The exact part played by the Gulf-stream in the heating of the North Atlantic Ocean and the western shores of Europe above the normal temperature of their latitude, though belonging more strictly to Physical Geography than to Geology, has yet an important bearing on the above subject. Dr. Carpenter believes that the direct influence of the Gulf-stream terminates with the latitude of the Bay of Biscay, and considers it an open question, whether the super-heating of the surface-water observed on a hot midsummer day beyond the northern border of the Bay of Biscay is not as probably due to the direct influence of the sun as to the extension of the Gulf-stream to that locality. This current he does not believe to extend to the Channel between the North of Scotland and the Faroe Islands. Professor Wyville Thomson, on the other hand, adheres to the older belief of the direct influence of the Gulf-stream on the climate of north-western Europe; an opinion which is shared by the eminent geographer Dr. Petermann, who maintains that there is no longer a shadow of doubt as to the existence of a movement of warm water, be it called a drift or a stream, from the Tropics obliquely across the Atlantic Ocean towards the Arctic regions. Dr. Carpenter states that the temperature-soundings taken in the "Porcupine" expeditions of 1869 and 1870, conclusively show that a temperature as low as 36·5° F. prevails over the deeper parts of the North Atlantic sea-bed; in connection with which the statement becomes of great interest which was made by Mr. Gwyn Jeffreys, at the recent meeting of the British Association, that the species of mollusca dredged up from great depths in the Mediterranean in previous expeditions are identical with Arctic species. An admirable *resumé* of the present condition of the Gulf-stream controversy, by Mr. Keith Johnston, jun., will be found in the "Academy" for Nov. 15th.

Principal Dawson, of Montreal, in the Bakerian Lecture delivered before the Royal Society, in May, 1870, described, under the name of *Prototaxites Logani*, what he believed to be the oldest known tree. The remains of this organism are abundant in the Lower Devonian or "Erian" rocks of Canada, and were described by Principal Dawson as "a simplification of the coniferous structure in the cylindrical cells marked only with spiral threads. They sometimes attain to a diameter of five feet, and though of a structure so lax as to be comparable only with the youngest stems of ordinary conifers, these trees must have been durable, as they are furnished both with medullary rays and rings of annual growth." More recently, however, Mr. Carruthers has made a minute microscopical examination of the structure of specimens of this wood, and has discovered that they are really specimens of hugh *Algæ*, belonging to at least more than one genus. They are very gigantic when contrasted with the ordinary *Algæ* of our existing seas; but some approach to them in size is made in the huge and tree-like *Lessonias* which Dr. Hooker found in the Antarctic Seas, and which have stems about 20 feet high, and with a diameter so great that they have been collected by mariners in those regions for fuel, under the belief that they were drift-wood. From a paper published in the October number of the "American Naturalist," it would appear, however, that Principal Dawson still holds to the coniferous character of *Prototaxites*.

In the recently published 2nd volume of Schimper's "Traiteé de Palæontologie Végétale," that eminent palæontologist divides the order *Lycopodiaceæ* into two families, *Lycopodieæ* and *Lepidodendreæ*, the latter entirely extinct, the former abounding at the present time and inhabiting all latitudes from the equator to the Arctic regions. Of this family Professor Schimper recognises only seven fossil species, all congeneric with *Lycopodium* itself, and, strange to say, confined, with the exception of one doubtful species, to the coal-measures. Dr. Hooker, on the other hand, considers that, after making every allowance for the imperfection of the geological record, it appears impossible to admit that a group so well represented now-a-days should be absent from all intervening beds, including the most modern Tertiaries; and is, on the whole, disposed to doubt the fossils being *Lycopodiums* at all.

The "Report on the Exploration of Kent's Cavern," presented to the recent meeting of the British Association, showed that during the past year the committee had investigated the only portions of the eastern division of the cave which had remained unexplored, called the North and South Sally Ports, in the belief that they led to external openings. The diggings yielded a large number of bones, including several birds and a few fish, portions of antlers, and about 1400 fragmentary and perfect teeth, some of them still attached to the jaw-bones. The teeth belonged to the following animals:—Horse, hyæna, rhinoceros, bear, sheep, badger, fox, rabbit, elephant, deer, lion, ox, hare, and pig. Agglutinated lumps of wings and elytra of beetles were also found; and about twenty-one flint implements and flakes.

The Geological volume of the scientific results of Professor Agassiz's Thayer Expedition, by Professor Hartt, of the Cornell University, discusses the origin of the layer of clay, or loam, varying in thickness from a few feet to one hundred, and wrapping in its folds the hills and valleys of Brazil, over vast tracts of country, including the steep slopes and summits of some of the highest mountains. It has been observed as far north as the Amazon valley, covering alike the gneiss and the Tertiary formations. It is of a red colour, evidently formed of the materials of the adjacent and underlying rocks, and without the slightest sign of stratification. Professor Hartt always speaks of this formation as "drift," and agrees with Professor Agassiz that its peculiarities are such as unmistakably to indicate its glacial origin.

BOTANY AND VEGETABLE PHYSIOLOGY.

Transpiration of Aqueous Vapour by Leaves.—Professor McNab, of Cirencester College, has recently published the results of an important series of experiments on this subject, confirming in a remarkable manner the statement made some time since by MM. Prillieux and Duchartre, that plants absorb no moisture whatever in the state of vapour through their leaves. The following are the more important results obtained :—1. Quantity of water in the leaves. The mean of several experiments gave 63·4 per cent. 2. Quantity of water removable by calcium chloride or concentrated sulphuric acid. This was found to be from 5·08 to 6·09 per cent, while the sun caused about the same quantity —5·8 per cent—to be transpired. The remainder, from 56 to 57 per cent. was therefore determined to be fluid in relation to the cell-sap of the plant. 3. Rapidity of transpiration in sunlight, diffused light, and darkness. The results given are—in sunlight, 3·03 per cent in an hour; in diffused daylight, 0·59 per cent; in darkness, 0·45 per cent. 4. Amount of fluid transpired in a saturated and in a dry atmosphere in the sun and in diffused daylight. In sunshine the experiments gave 25·96 per cent in an hour in a saturated atmosphere; 20·52 per cent in a dry atmosphere; in the shade, none whatever in a saturated, 1·69 per cent in a dry atmosphere; thus again confirming the earlier observations of M. Dehérain, that in sunlight evaporation goes on equally in a saturated as in a dry atmosphere. 5. Quantity of water taken up by leaves when immersed in it. The mean of several experiments gave 4·37 per cent in $1\frac{1}{2}$ hours. 6. Quantity of aqueous vapour absorbed by leaves in a saturated atmosphere. None whatever. 7. Differences in the amount of fluid transpired by the upper and under side of leaves in the sun and in diffused daylight. Under both circumstances the amount was found to be much larger from the under than from the upper surface. 8. Rapidity of ascent of fluids in plants. From $4\frac{7}{12}$th inches in 10 minutes to $8\frac{7}{12}$th inches in 70 minutes. In all these experiments the plant operated on was the common cherry-laurel (*Prunus laurocerasus*), and the fluid used for testing the rapidity of the ascent was lithium citrate, the presence of a very small quantity of which can be readily detected by the spectroscope.

The phenomena of cross-fertilisation and self-fertilisation are still attracting much attention among botanists. Mr. A. W. Bennett details in the "Journal of Botany" for October, a series of observations on British plants. Two sets of facts have been especially observed, in particular by Darwin in this country, Hildebrand in Germany, and Delpino in Italy, to favour cross-fertilisation in hermaphrodite flowers; the phenomena of dimorphism and trimorphism, and the special arrangements which render it easier for the pollen to be brushed off by insects visiting the flower than to fall on its own stigma. But, besides these, another arrangement exists by which self-fertilisation is hindered, the single fact that the stamens and pistil belonging to the same flower are frequently not ripe, so to speak, at the same time. The terms *protandry* and *protogyny*, used by Hildebrand, are adopted by Mr. Bennett; but instead of non-dichogamy, he prefers *synacmy* for the simultaneous maturing of the two sets of organs. The most frequent arrangement appears to be that the pollen commences to be discharged from the anthers at a longer or shorter interval before the maturing of the stigma. In some cases there still remains a certain quantity of pollen in the anthers when the stigma is ready to receive it; in other cases the anthers have either withered up or entirely dropped off before fertilisation of the ovules can possibly take place. Synacmy is nearly as frequent as protandry; while protogyny is of far less common occurrence. The two extremes among the species observed may be stated to be *Campanula rotundifolia* and *Scrophularia aquatica*.